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BEHAVIOR VERSUS INTROSPECTIVE PSYCHOLOGY [1]

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Since the clear formulation of Behaviorism by Watson,[2] many articles and much space have been given to reconcile this position with the older viewpoint of psychology as the science of consciousness. Perhaps the most enthusiastic author who has attempted to make this reconcilation has been Weiss.[3] Few psychologists seem to have been willing to stand either as out-and-out Behaviorists or as out-and-out Introspectionists. Take up any textbook of psychology extant, with the exception of Watson[4] and you will find both introspective and behavoristic material treated frequently side by side.

It seems worth while to decide what may be properly treated within the limits of a single science. We believe that the method of all science is primarily observation of an individual datum *from a given point of view and with a tentative*

[1] The present paper contains largely the conclusions arrived at after a year's discussion of psychological systems in the Psychological Seminar at the University of Pennsylvania.

[2] Watson, J. B., 'Psychology as the Behaviorist Views it,' PSYCHOL. REV., 1913, 20, 158.

[3] Weiss, A. P., 'Relation Between Structural and Behavior Psychology,' PSYCHOL. REV., 1917, 24, 301–317; 'Relation Between Functional and Behavior Psychology,'' PSYCHOL. REV., 1917, 24, 353–368; 'Conscious Behavior,' *J. of Philos., etc.*, 1918, 15, 631–641; 'Relation Between Physiological and Behavior Psychology,' *J. of Philos., etc.*, 1919, 16, 626–634.

[4] Watson, J. B., 'Psychology from the Standpoint of a Behaviorist,' 1919, Philadelphia, pp. xii + 429.

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set of categories into which the individual observation is to be classified. Science thus becomes a 'house-that-Jack-built' sort of affair. One makes observations of individual bits of datum which are then classified into categories. *With these categories in mind,* new observations are made from which new categories are formulated or the older ones corrected, so that still further observations can be made, and so on. Science thus becomes an alternating series of direct observation and of speculative systematization and evaluation of data thus secured. And the point of view of any science becomes merely the system of categories from which the observations are made. These define, not only the point of view, but also the materials to be properly treated by the science and the methods to be used.

The older and, with few exceptions, the present psychologists are interested in three general terms: (1) stimulation, (3) reaction and (2) the intervening mental process. The true out-and-out introspectionist should, by the definition of his point of view, be interested solely in the stimulation and mental process parts of the chain. He should properly have no interest whatsoever in reaction. On the other hand, the ideal behaviorist wishes to drop the middle term of the intervening mental process entirely and should be interested solely in the two ends—stimulus and reaction.

Let us contrast the two disciplines briefly and without too great a degree of finality. The materials to be studied by a thoroughgoing behaviorism are the reactions of the striated and non-striated muscles and of the glands and their integrations. One must certainly also include a study of biological meanings.[1] The method is one of simple observation, either directly or in a more refined manner with the aid of instruments, on the part of the experimenter or experimenters with a classification of the observation into interpretative *reactive* categories. The observation need not be confined to a single investigator. By the simple classification into interpretative categories, the *qualitative* system of behavior may be formulated directly. Data such as these may readily lend

[1] *Cf.* Titchener, E. B., 'Textbook of Psychology,' 1912, 369 ff.

themselves to statistical treatment. Hence we would include most of the quantitative work on memory, forgetting, sensation, association, learning and the like under the behavioristic discipline.

The other discipline we would call the *Science of Consciousness*. The material of this science is merely the conscious middle term in the reaction chain and nothing more. In this we would include a study of the conscious elements, of their integrations and of conscious meanings. The method is systematic introspection—experimental wherever possible. Hence the subject is also the experimenter. The observation is entirely individual. Such data readily give a qualitative system of consciousness, but they do not lend themselves so readily to statistical treatment. Hence the science of consciousness must be largely qualitative in form. We have not considered here the question regarding the invalidity of introspection at all. Being a 'dyed-in-the-wool' introspectionist, the problem does not exist for me any more than it does for other introspectionists who have had the opportunity to be trained and who have used the method. Observation in both sciences is entirely disinterested.

We advocate, then, the separation of the present psychological discipline into two distinct and independent sciences. Both have their point of view, both have their different interpretative categories, both have their distinct materials for observation, and both have their methods. Furthermore, both may be truly experimental and each may consistently be carried on from a single point of view. But one must realize that methodological training is equally important for observing in the science of consciousness and for the science of behavior. In the science of behavior, the experimenter must be trained, not only to observe, but to observe under a group of interpretative categories. In the science of consciousness, the subject is also experimenter (a dictaphone will do as well as a human being for the recording) and so the subject must also have the training as observer.

This splitting of the present-day psychology into two distinct sciences is not new in the history of thought. Take

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the case of chemistry, for example. Early chemistry started as a branch of physics. Then, after it had established its right as an independent science, methods and points of view were developed so that today the independent sciences that have developed from chemistry are numerous and some (for example, mineralogy, crystallography and metallurgy) have even lost the chemical connotation. Such a development is to be expected with the formulation of new problems and the invention of new methods.

But in this frank recognition of the science of behavior and of the science of consciousness as independent disciplines, what becomes of the old psychology? It still remains as a perfectly proper field of endeavor. It remains as an attempt to correlate the data obtained from the two observational sciences and also, usually, to correlate these facts with physiological data. In that case, this discipline is *not* a science but becomes a philosophical, speculative procedure. It is, indeed, the last step of correlation and it properly should draw facts, not directly from either science, but from a group of borderline sciences which must start, and actually have started to develop. Hence the first step of correlation is the development of the sciences of behavior-physiology; of consciousness-physiology and of behavior-consciousness. These are sciences because they are observational—inasmuch as one may study a temporal correlation at least—and they must be carried on by persons trained in the methods and in the interpretative categories of at least two of the fundamental sciences.

Furthermore, either of the two sciences of consciousness or of behavior may have contact with a great number of other sciences or of systems of speculative interpretation. Thus a functional behavior may be developed as well as a functional view of consciousness, inasmuch as a teleological explanation may be given to either system of observed facts.

We believe, then, that the present data treated under the term psychology cannot be treated from a single point of view or from a single set of interpretative categories. In the present state of our knowledge, two observational points of view seem to be sufficient. We would frankly recognize,

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therefore, the independence of the sciences of consciousness and of behavior. The two are so different in point of view, in interpretative categories implied, in materials to be studied and in methods to be used that a reconciliation under one science would seem impossible.[1] The older psychology emerges, thus, as a form of speculative procedure attempting to correlate the data of these two independent sciences with each other and with the data of physiology.

[1] For an excellent selected group of references for such unsuccessful attempts, *cf.* Tolman, E. C., 'A New Formula for Behaviorism,' PSYCHOL. REV., 1922, 29, 44–53.

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THE UNIQUE IN HUMAN BEHAVIOR

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Humans run, walk, grasp, wink, climb, turn the head, bend the body, chew, strike, crouch. But other animals do all these things. So far as concerns these minor units of behavior there is no distinction between human beings and other animals.

Suppose we note how these minor units of behavior are connected together into larger systems of action. We see that the walking, grasping, hitting, climbing, chewing are all connected in such a way as to procure food, or, it may be, to escape from danger, or to obtain shelter from wind and rain, or to foster the young. We may designate these larger systems by the term *instinct*, in order to distinguish them from the minor units of behavior out of which in part they are made. Instinct is a word with many different connotations, but for the present we shall mean by it nothing more than we have already indicated, namely, an organized system of minor units of behavior under the control of an underlying propensity or determining adjustment.[1]

Have we here something with which to distinguish human behavior? Plainly not. In the lower animals also the minor units of behavior are integrated into larger systems which serve to procure food, escape from danger, foster the young, and destroy the foe.

But if we continue to compare men and beasts we shall find that in men these instincts are themselves organized into still larger systems which we shall call *interests*. These do not appear in the behavior of beasts. Humans procure food, escape from danger, fight enemies, seek association with others of their kind, even as the lower animals; but they

[1] Instinct has been well defined by E. C. Tolman as determining adjustment. PSYCHOL. REV., May, 1920, 27, 217. Article entitled 'Instinct and Purpose.'

do all these things in such manner as to make of their doing a system of behavior even more comprehensive and complex than that of the instincts. It is these interests which distinguish human behavior from everything else in the world. In other words, human behavior is more complex.

It should be noted that thus far nothing has been said to indicate that there is any other difference save that of increasing complexity between the three levels of behavior which we have called minor units, instincts, and interests. Reflexes are integrated into minor units of behavior; minor units are integrated into instincts; instincts are integrated into interests. Thus far we have distinguished these three quantitatively rather than qualitatively. We shall find a little later that a unique quality distinguishes the working of interests. But we wish to emphasize that our classification thus far does not depend upon any such qualitative distinction. We emphasize this because we believe all will agree thus far and we must have this common ground of agreement before we can profitably consider other points over which disagreement may arise.

Of course what we call interest might by others be called instinct. But the point we want to make is that certain unique types of behavior, found among humans, and wholly different from the instincts of lower animals, have a unique and independent function over and above instinct as it appears among the lower animals. To make this point clear we must have another term than instinct with which to designate these unique types of behavior.

The innate constitution of human nature does not now concern us. But to clarify our concept of interest we must state whether or not we represent interest as an innate endowment of human nature. Interests are not innate as that term is frequently interpreted. But the development of interest in human beings, and its failure to develop in other animals, is not accidental. It is due to certain peculiarities in the immature human organism. These peculiarities are such that the human will inevitably develop interests under normal conditions while other animals just as certainly will

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not. In this sense, but only in this sense, can it be said that the human has an innate tendency to develop interests as well as instincts. What these peculiarities may be, which give rise to interest, is not now our theme. We wish now merely to describe the distinctive characteristics of interest.

Any system of behavior must be characterized by what it is doing. The minor units of behavior we noted are doing such things as chewing, climbing, grasping. The instincts are doing the work of procuring shelter, caring for the young, escaping danger. But we have not yet stated what the interests are doing. Are the interests doing anything more than support the instincts? Is interest solely instrumental to the satisfaction of instinct or does it have an independent function? This is our crucial question.

Many answer this by saying that the complexity of human behavior is due to the artificial conditions under which we live. The direct fulfilment of instinct is the natural way of life for man, but the artificial and complicated situations imposed by civilization force the instincts into the complicated patterns which we have called interest. Hence interest represents no value independent of instinct. The behavior called interest is merely the elaborate psychic machinery with which the instincts must work under civilized conditions.

This view we believe erroneous. It confuses cause and effect. How did these artificial and complex situations arise? They are not merely the cause of man's complex behavior; they are most certainly also the effect of man's peculiar way of acting. Civilization is not merely something imposed upon human nature and causing human behavior to develop more complex systems of action than those of the instincts. On the contrary civilization is also most certainly the product of human behavior. Many factors contribute to the development of civilization, but human behavior is one.

The question before us is this: What is the difference between what instinctive behavior is doing and what interested behavior is doing? Our answer is that instinctive behavior is adapting the organism to some specific situation,

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while interested behavior is adapting the organism to an ever more inclusive situation. Since there is no limit to the including situations, interested behavior is progressive adaptation which is never complete. It is adapting the organism to the world in its totality. Instinctive behavior is complete when adaptation to the specific situation has been accomplished, such as escape from danger or acquisition of sufficient food. But in case of interested behavior there is no such status to be attained; there is always something more to be attained. There is always a more inclusive situation to which adaptation must be made. Not only food for today but also for tomorrow. Not only food for tomorrow but a variety of foods. Not only food in variety but food with adornments, and with a place in which to store it, and with pleasant companions with whom to eat it. Thus interested reaction to food is reaction not only to the food but to the food as related to ever more inclusive situations until at last the total destiny of the world is the inclusive situation to which one reacts in eating food. Hence religious ceremonial must accompany the repast.

This difference between the doing of instinct and interest may be otherwise expressed by saying that instinct is making this or that particular contact with environment while interest is making simultaneously every possible contact with environment. Instinct either eats the apple or observes its bright red color. But interest makes innumerable contacts with the apple. Interest eats the apple and also keeps it in such a manner as to observe its bright red color. This it does in most primitive fashion by recalling in memory the bright red apple after it has been eaten. Thus it is that humans develop myth and tradition about apples. They describe and draw, paint, write poetry, and sing songs about apples. They develop a science that investigates the apple from every possible angle and an art of horticulture that cultivates it.

Instinct flees from danger, but interest both flees from danger and at the same time courts danger. It is interest, not instinct, that man displays when he plays with danger; goes to the ends of the earth to find it; maintains bull fights

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and delights in story, song, and drama that depict the terrible and even the horrible. Nothing in the world is a matter of indifference to interest. Whatever is, may be the object of interested reaction. Not so with instinct. Instinctive behavior follows a certain orbit, as it were, making only those contacts with environment which happen to fall in the path of its orbit. Interest, on the other hand, follows contacts, making every orbit that will yield further contacts. According to instinctive behavior the animal flings about until it hits upon some satisfying contact, every other contact being abandoned for the sake of this satisfying contact. But for interest every contact may be satisfying. Hence the human deliberately and continuously flings about. In consequence he is the great discoveror and inventor in the world.

To say that every contact may be satisfying in interested behavior may raise grave doubts. We are not saying that human behavior makes innumerable contacts regardless of the quality of that contact. We are only saying that in so far as human behavior is unique, *i.e.*, interested rather than instinctive, it does so. But human behavior is not always interested. It is sometimes instinctive solely, for human nature comprises both instinct and interest and may display the one without the other. Furthermore, the fact that in interested behavior itself certain contacts are avoided or neglected, and others much preferred, does not disprove our thesis; for certain contacts may be avoided because they do not lead to any further contacts while others may be much preferred because they give promise of a rich array of future contacts. Further evidence for our position is found in the fact that contacts[1] that are unendurable to the instincts and minor units are cautiously approached in interested behavior. Also these unendurable contacts are depicted

[1] The term 'contact' may not be the happiest for our purpose. We mean by it experience. We also mean responses. To say that every contact is satisfying is to say that interested behavior is that behavior which is always in process of developing a more complex system in response to the total field of experience, as distinguished from that response to limited fields of experience which is instinctive. Interested behavior is not only more complex than instinctive, which is merely a difference in quantity but it is ever developing a greater complexity, which is a difference in quality.

with much appreciation by many forms of art. Tragic art in particular does this.

Contrast the instinctive and the interested behavior in the care of the young. Instinctively the animal does what is necessary to protect and foster the young. It does very little that is unnecessary. Or if it does do that which is unnecessary the unnecessary activities tend to disappear. But in interested care of the young the unnecessary activities are most conspicuous. The infant is bedecked with lace and ribbon. It is talked to and sung to and dressed and undressed. It is put in a room with pictures and rugs and chandeliers. It is presented to guests and its birthday is celebrated. It is subjected to ceremonies, not least of which is religious baptism. Interested behavior reacts to the infant as the center of the universe literally. By this we do not mean merely that it is much beloved. We mean that interested behavior is reaction to the infant as the center of the universe. One reacts to the universe as the frame in which the infant is set. Instinctive behavior is reaction to the infant pure and simple, the universal frame of things not entering into the stimulus. Animals do not baptize their young nor in any other wise present them to the gods of universal destiny. Interested behavior, in dealing with the young deals with the universe. Instinctive behavior deals only with the young. Humans, of course, are not always interested. They are often merely instinctive.

This same difference between instinct and interest may be noted in food-taking. The behavior of the hungry animal devouring food is not modified by the cut glass, the snowy linen, the flowers, the music, the presence of associates, the bright cheery room, the landscape visible through the window, the pictures on the wall, except as these directly frustrate the attempts to get the food. But in eating food interestedly one is reacting to a world of things of which the food is but one part. Interest is always reaction to a world full of all sorts of things from cabbages to kings. Instinct is reaction to some particular object, the world being otherwise non-existent.

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Compare human play with that of lower animals and the same contrast is apparent. The play of beast is instinctive behavior; human play is interested behavior. Of course humans may play as do the beasts but they also have a very unique way of playing. Karl Groos and his followers have never recognized this wide difference between human play and the play of beasts. The play of the beast is the exercise of immature instincts, true enough. A puppy never plays that he is anything save a little dog. He never plays that he is a horse or a man or a duck or a star or a tomato or the wandering wind. But a child does play that he is all these things and much more. Simply to enlarge the field of experience is the characteristic of human play. To be sure these multiplied contacts are not merely at random. Human play as a rule is orderly, but it is not the order of instinct. Or would one maintain that when a child plays he is a polar bear he has an instinctive craving to sit on ice? Or when he plays he is a horse there is a peculiar instinct that impells him to stand beside a stall and eat hay? No, the order of human play is the order of culture rather than the order of instinct. Culture is the integration of the maximum number of contacts with environment regardless of the quality of these—with one exception. The only contacts which culture excludes are those which it has not yet been able to integrate with the prevailing system. But this is a deficiency in the system against which human nature rebels, as we shall show later. Human play is the child's response to the world in all its fulness. Culture is the integrated form of such response, hence play assumes the forms of the prevailing cultural system, or of some cultural system.

Human curiosity also reveals the uniqueness of interested behavior. The lower animals show curiosity also, but it is instinctive, not interested. The lower animals are curious only concerning certain definite objects. Their curiosity might be called biological in as much as it is little more than the perceptive aspect of those forms of behavior which are indispensable to biological survival—the instincts. In the case of monkeys, and perhaps in a few other animals, curiosity

seems to pass beyond this. Yet even in the monkey curiosity never attains that independence of biological demands which it displays when it becomes human. The monkey does not investigate the rings of Saturn nor the theory of relativity nor pursue the higher branches of mathematics.

This interpretation of interested behavior will explain much which the Freudians are telling us about the "Unconscious," we believe. The Unconscious has been tried and convicted of much wickedness. It is called the primitive, archaic, infantile, uncivilized, gross, and titanic. The social conventions and cultural life of man have been superimposed upon this crude craving unconscious propensity of human nature, they say. But how have these social conventions and this culture arisen? We opine that this craving unconscious which chafes against the cultural systems reveals that in us which has given rise to these very cultural systems.

The unconscious is nothing else than all those reactions to environment which cannot be integrated into the cultural system. The beast is not troubled with an unconscious after the fashion of man because he is not constantly reacting to so many different things at the same time. It is only interested behavior that displays a great number of antagonistic responses because interested behavior is precisely reaction to the greatest possible number of stimuli, while instinctive behavior is reaction to only a particular set of stimuli. Only humans develop a culture because culture is the integration of originally diverse and antagonistic reactions so numerous that nothing save the artificial system of culture could integrate them. Culture is that system of behavior which provides with minimum of conflict for the greatest number of diverse responses. But no culture has yet been developed which has done this sufficiently well. Hence there is always a residuum of responses for which the cultural system makes no provision at all or no adequate provision because the interested human (not the instinctive human) is always responding to environment in the greatest possible number of different ways. This residuum is what makes the unconscious. Yet it is precisely this residuum

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which is the promise and potency of all further development of culture. Human progress may be measured by the degree to which the prevailing cultural system makes possible a complex response to the world in its totality.

It should be noted that the value of culture is not primarily in the fact that it removes conflict. The conflict arises out of something more fundamental, and the value of culture is measured by the degree to which it meets this more fundamental demand. Humans have the tendency to respond to everything. Conflict of responses makes it impossible to respond to everything. In so far as culture removes conflict it makes possible response to a greater number of things. There lies its value. Why is it of value to respond to everything, or to the world in its totality? Because the instincts are thereby more amply fulfilled? No, they are not thereby more amply fulfilled. But it is of value to respond to the world in its totality because that is the fulfilment of human nature regardless of any other utility or disutility. To respond to everything in its totality is the unique in human behavior, or interest.

Conflict may be removed not only by integrating conflicting responses but also by dissociating them, if the dissociation is complete. As a fact the historic cultures have removed conflict in both these two ways, by integration and by dissociation. But dissociation is never enduringly complete. It is dissociation which yields the troublesome unconscious. Human nature persists in responding to a greater portion of the world than that for which the cultural system has provided. It is only as the cultural system provides for the integration of all responses that are made to the whole accessible world, that it is an altogether successful culture. In so far as a developing culture does this we have progress.

We must repeat the denial we have previously made: Our thesis is not that human behavior is always interested. We might even say that the unique in human behavior is that which the anthropomorphic beast tends to display, rather than that which he always manifests. Human behavior may be a level which this sort of animal sometimes attains and

sometimes does not. We think, however, that interested behavior is far more common among men than is usually recognized. True human behavior has been slurred over by the widespread attempt to interpret our conduct in terms of instinct like that of the lower animals. The other alternative has been to represent human nature as something mysteriously and supernaturally spiritual. We believe both these views wrong. Humans possess all that the beasts have plus something more. This something more may be weak or strong as compared to the animal nature. In some, interest may be strong as compared with instinct; while others may behave more like beasts than men. Also it is plain that all sorts of maladjustments and perversions may arise in a being so complex as to comprehend in his nature both instinct and interest. But healthy normal instinct and healthy normal interest are clearly distinguishable. Instinct approaches or flees from certain specific situations. Interested behavior both approaches and flees at the same time. In case of fire the beast flees or at best remains at a respectful distance. The human both flees and approaches—by putting on asbestos gloves.

Instinct and interest do not necessarily conflict, although they may. It is all a matter of organization. If the instincts are rightly organized they are not only the components, but the indispensable servants, of interest. Any organization adequate to interested behavior must be a most plastic organization, subject to all manner of delicate modifications and always undergoing further evolution. But instinctive organization is not always plastic. Rather, instincts tend to stereotyped habits and thus resist the progressive organization that constitutes interest. Of course an interest system may be plastic even when the instinctive components of the system are rigid, but the more rigid the latter the more difficult the organization and the more likely are conflicts to arise.

Let us summarize our treatment of human behavior and at the same time suggest some of its further implications. Behavior of lower animals is determined by anticipation of

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certain specific situations. Human behavior is determined by anticipation of a great number of possible situations ultimately included in that total situation called the world. Philosophy is the deliberate attempt to define this totality of situations. Religion is personal response to these in their unknown totality. Science deliberately limits this totality to certain very narrow bounds in order to investigate that field intensively. Art is the symbolic representation of unexplored possibilities of experience. Love is personal response to a vista of unexplored possibilities to be realized through converse with the beloved personality. These are all unique traits of human behavior.

ANALYZED VERSUS UNANALYZED EXPERIENCE

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In a recent number of this Journal [1] Professor Calkins has criticized at length my "Experimental Investigation of the Process of Choosing" [2] and my subsequent paper on "Theories of the Will and Kinæsthetic Sensation."[3] Chief among her assertions are the following: (1) I have inferred that the volitional consciousness consists entirely of kinæsthetic sensation; (2) I have neglected certain evidences of 'self-activity' in the introspective reports of my reagents and have thus failed to describe one of the major components of the volitional consciousness, namely, consciousness of the self; (3) I have failed to meet the results of Ach [4] and Michotte [5] who found in the volitional consciousness an experience, associated with consciousness of the self, which they designated as a *Bewusstheit* of willing and as a feeling of mental activity (conscience de l'action) respectively.

Miss Calkins' criticisms rest upon the assumption that Ach's and Michotte's introspections were more accurate with respect to important details than my own. They are also based upon the belief that the experience, consciousness of self, contains contents which are not to be reduced to sensory and imaginal data. Since she has refused to accept my experimental results as sufficient contrary evidence it will be necessary to precede a direct rejoinder with further data and argument.

The purpose of this paper, therefore, is to attack the introspective basis upon which Miss Calkins' position rests

[1] Psychol. Rev., 28, 1921, 356–373.

[2] University of Oregon Publications, 1, 1920.

[3] Psychol. Rev., 27, 1920, 351–360.

[4] Ueber Willensakt und das Temperament, 1910.

[5] Étude expérimentale sur le choix volontaire et ses antécédents immédiats. *Arch. de psychol.*, 10, 1910, 113–321.

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and to show that her third assertion, as given above, is erroneous.

I shall attempt to show: (1) That Ach's and Michotte's observers were for the most part describing unanalyzed but not unanalyzable experiences; (2) that their introspections were faulty because they consisted so largely of interpretations from unrecognized sensory and imaginal complexes; (3) that they experienced sensory and imaginal processes in the volitional consciousness as did our reagents but were unable to analyze all of them; (4) that when a volitional experience is unanalyzed it appears, in part, as the *meaning*, 'I will,' or 'feeling of activity,' or in the form of the more refined meaning, 'motor adjustment,' depending upon the disposition of the reagent to interpret his experiences in one fashion or another; (5) that when a reagent assumes a sufficiently analytical attitude the experiences in the volitional consciousness do not necessarily mature to the extent that they mean 'I will' and 'feeling of mental activity' but that they remain in the form of basal meanings—sensation and image; (6) the differences between the conclusions of Ach, Michotte, and myself accrue to the fact that the data of the first two investigations were unanalyzed meanings while the data of my investigation were analyzed meanings.

Miss Calkins' case would have been much stronger had she told us what, in her estimation, constitutes a *reliable* introspection. As it is, one might suspect that her clue to the proficiency of Ach's and Michotte's introspections is the fact that they corroborate her self-psychology! Be it remembered that Ach was content to summarize his introspective data; Michotte published excerpts only.

Let us actually see what Ach's data were like, bearing in mind that we must judge from his summaries. Here is an example which has the ear-marks of an original introspection.

After reading the stimulus-syllable, 'kon,' in inner speech with which was experienced a feeling of familiarity, there came the awareness that an inappropriate syllable would appear; then in inner speech came the associated syllable 'dub,' and a persisting *Bewusstseinslage* of confusion. Then the awareness (*Bewusstheit*), 'I

will repeat some other syllable;' immediately following this the letter 'p' was visualized in place of the 'k,' together with the definite *Bewusstheit* that I had already reacted with these letters in the last experiment. Then I innervated the syllable, 'pon,' which came with the *Bewusstheit* that the reaction was correct.[1]

Again, we read: "Thus in our experiments, the *meaning*, 'I will construct a rhyme as quickly as possible for the syllable which is about to appear,' accompanied the word 'rhyme' as the latter came in inner speech."[2] In another instance we are told that the verbal imagery (we are not able to report what kind of verbal imagery) 'quickly and correctly' appeared in the reagent's consciousness together with the *awareness* (*Bewusstheit*), 'I will construct a rhyme quickly.'[3] Elsewhere a *Bewusstheit* is called the 'indistinct knowing' that 'I will try to make no mistakes' and that 'I ought and will construct another rhyme and know how to do it.'[4]

Now we are told by Ach himself that a *Bewusstheit* is 'complex conscious content simultaneously present as knowledge'[5] and that it is an awareness of meaning. These meanings, as is demonstrated above, often run to great detail. We are led to believe that these meanings are non-sensory and imageless yet they are complex for the quotations given above and many others tell us the contents of these meanings! The question is: Are these contents analyzable into sensory and imaginal processes and are these meanings nothing more than interpretations from basal sensory and imaginal experience?

It has been pointed out that meanings are analyzable and that when we report a sensory and imaginal experience as meaning so and so we are interpreting, for we are telling the experimenter something more than we experienced originally.[6] That a *Bewusstheit* is just such an *interpreted* sensory and imaginal experience is evident from the following account of an experiment from the writer's laboratory. This experi-

[1] *Op cit.*, p. 158.
[2] *Op. cit.*, p. 239.
[3] *Op. cit.*, p. 240.
[4] *Op. cit.*, p. 159.
[5] Ueber die Willenstätigkeit und das Denken, p. 210.
[6] Wheeler, *op. cit.*, pp. 29, 30, 31, 33, 51, 52. 'The Development of Meaning,' *Amer. J. of Psychol.*, 33, 1922, 223–233.

ment also demonstrates that the meanings called *Bewusstheiten* are analyzable into sensory and imaginal contents.

The writer was studying the difference between unanalyzed and analyzed experience and was subject in a reaction experiment. The easiest and most natural introspective report was given first as follows:

"In the foreperiod I was conscious of the fact that I was to react as quickly as possible and that the stimulus would appear on a white card."

The above is a genuine report of 'complex conscious content simultaneously present as knowledge;' in other words a *bona fide Bewusstheit.* That this report, however, was a statement of unanalyzed experience is demonstrated by the following description of the same experience *analyzed.*

"There first appeared intense muscular strains, localized in the fingers and arm of the reacting hand; strain of eye-fixation toward the screen; visual perception of the screen and of the surrounding apparatus; then a momentary shift of attention to a mass of bodily kinæsthesis about the neck, trunk, and shoulders. This kinæsthesis was visualized and localized as it appeared in consciousness; then came a momentary broadening of the span of attention taking in this mass of visualized musculature, the screen, and a fleeting, sketchy visual image of whiteness with a black streak across the center, projected beyond the black screen."

How different is the description of the unanalyzed experience from the description of the same experience when analyzed!

Let us now return to the first 'introspection' which is nothing more than a report of *Bewusstheiten* (statements of meaning) and inspect it for accuracy. This, the reagent said, was his awareness: "that I was to react as quickly as possible and that the stimulus was to appear on a white card." The facts of the matter were these: The nearest approach to an actual consciousness of the self was the broadening of attention until it involved the mass of kinæsthetic sensations, visual localizations of these sensations, and visual perception of the experimental situation. This complex consciousness was only the beginning stage of an interpretative period which might have developed at the time into a consciousness of the self but did so only after the original consciousness had run its course and the reagent was beginning to intro-

spect. The meaning, 'react,' was not a datum of the original consciousness at all; it was another interpretation and a statement of what the kinæsthetic consciousness might have meant to the reagent at the time. The meaning, 'quickly as possible,' was nothing more nor less than an interpretation from muscular tenseness of the arm and fingers and from muscular strains from a rigid visual fixation of the screen. The meaning, 'and,' was not present to consciousness originally; the meaning, 'would appear,' was another fictitious process; both of these meanings appeared only when the introspection was being innervated verbal fashion. The meanings, 'stimulus' and 'white card,' were likewise afterthoughts—interpretations from a flashy visual image of whiteness with a black streak across it, projected behind the screen.

It is obvious that the first description was an extremely inaccurate account of what was actually experienced at the time. Many of the implied meanings appeared to the reagent as he was preparing to introspect; others came as the introspection was being dictated to the experimenter. The reagent was elaborating upon his basal sensory and imaginal experiences.

There is sufficient evidence of these facts, for while the reagent was in the act of introspecting he found himself deriving meanings from his original and analyzed experiences. Along with the process of focusing his attention upon *immediate content*, so-called, the reagent discovered, tapering off in consciousness, fleeting vocal-motor-auditory imagery of the words: 'fact,' 'react,' 'quickly,' 'possible,' along with a mass of other vocal-motor-auditory imagery which amounted to nothing more than rapid, rhythmic changes in throat tensions. Further, he caught himself delineating the entire original experience by means of a rapid, sweeping, visual perception together with visual imagery; this wave of expanding visual attention took in the entire experimental situation momentarily, including the reagent's motor attitude. In this sweep of attention certain features stood out as nodal points: (1) a momentary pause of his line of regard

upon his chest and arm while kinæsthetic sensations increased in clearness; (2) a momentary pause of his line of regard upon the screen; (3) a momentary pause upon the visualized whiteness behind the screen.

Here we obtain a cross-section of original and basal experience running its course in the development of meaning. A *Bewusstheit* is in the process of formation. In the absence of an analytical attitude at the time, this *Bewusstheit* developed to a full-fledged consciousness of meaning but this development of meaning, rapid as it was, could be described introspectively.

From a functional standpoint, then, the *Bewusstheit* was a rapid summing-up of sensory and imaginal experience; it was a complex 'interpretative period.' Basal sensory and imaginal contents were rapidly shaping themselves into meanings and these meanings were ultimately to take the form of innervated words. Immediate experience, so-called, was rapidly becoming knowledge—interpreted experience, complex contents simultaneously present as knowledge. The simultaneity of this knowledge appeared in 'cognizing' the experience 'synthetically.'

All of this was observed fact and was discovered when the reagent really introspected both upon his original mental processes and upon the subsequent act of deriving meanings from these contents while the latter were running their course in consciousness. Thus, from the time that the reagent had prepared himself to react to the time he had innervated the introspective report, four major activities were taking place: (1) The original experiences whose contents were sensory and imaginal; (2) the development of these experiences into unanalyzed meanings, leading to a narrative form of 'introspection'; (3) introspective analysis of these developing meanings; (4) introspective analysis of the sensory and imaginal contents involved in the original and basal experiences. In none of these activities were to be found any mental processes whose contents were not sensation and image.

All of this shows that unless an introspector is sufficiently trained to detect the differences between gross interpretation

and refined analysis he will report unanalyzed experience and not analyzed experience. Moreover, unless he succeeds in distinguishing between secondary meanings and basal contents, he will never know that he has failed to describe all that was actually present. He will erroneously accept *Kundgabe* for a final analysis and will persist in the faulty belief that he has accomplished his introspective task.

Let us now turn to typical introspections from Michotte's investigation. It will be recalled that his reagents were to choose between the alternatives of multiplication and division or of addition and subtraction. Simple arithmetical problems, printed upon cards and exposed in a tachistoscope, furnished these alternatives.

The following excerpt deals with the inspection of two alternatives.

> First of all I had a feeling of astonishment; this series of numbers seemed ridiculous. Then the impression that division had the favorable advantage, without knowing on what I based this idea. It seemed as if I were vaguely recognizing that the result would then be small. I hesitated an instant and thought, "No, it ought to be more complete." Immediately I remarked to myself that a portion of the number to the left was divisible by the number to the right. Then the word 'multiplication' appeared along with the idea that it (division) would be a more complicated procedure than appeared at first.[1]

Here is a choice which lacked vigor.

> The word 'multiplication' appeared in questioning inflection. Then inspection (of the alternative) was halted during which time I found myself in a state of doubt; but there were no concrete processes present. Suddenly the feeling changed from doubt to certainty and with the certainty came the word 'division'; this appeared as the natural outcome of the experience which was thus far developing; the word also appeared as if it were a decision already made. The choice was more than a simple designation of the word, 'division'; it was an affirmation, for the word designated was thus found fixed and definite when it appeared in consciousness.[2]

Here is a vigorous choice.

> The feeling of *to do, to designate* (I am able to express myself only by employing the infinitive) accompanied the decision. The decision and this 'acting' were one and the same thing. There appeared a consciousness of deciding. I do not find that this consciousness exists in different degrees as do feelings of muscular activity; the thing is done by me; it does not accomplish itself; there is no middle course. I find myself spontaneously using the word 'feeling' to express this experience; I can describe it only in terms of 'feeling of activity.'[3]

[1] *Op. cit.*, p. 160.
[2] *Op. cit.*, p. 188.
[3] *Op. cit.*, p. 193.

Here are typical introspections teeming with unanalyzed doubts, certainties, affirmations, ideas, verbal processes, thoughts, recognitions, impressions, hesitations, remarks, designations, and feelings of activity. Do we need to point out that all this is statement of meaning and not analysis? One could turn to almost any novel and there find just as good unanalytical statements of experiences and reports of information as these. They are narrations and not analyses. Introspective technique has far surpassed this unanalytical stage. It is extremely unscientific to refer to such data, as Miss Calkins has done, for confirmation of the detailed facts of experience.

Should the introspections in my experimental study on choosing be insufficient evidence of my claims, let us turn to another introspection from the author's laboratory. The reagent, again the author, was asked to choose between the alternatives of addition and subtraction in the following problem:

$$\begin{array}{r} 258 \\ 135 \\ \hline \end{array}$$

First will be presented the reagent's statement of his unanalyzed experiences which corresponds very well with Michotte's introspections.

I had the impression that the upper figures were large and that the lower figures were small. Thought at first that I would decide to add but changed my mind and finally decided to subtract. I knew that I was doing the deciding; during the decision I had a feeling of mental activity. There was also a consciousness of being very deliberate and serious in making this decision. I chose for a serious reason and felt, afterwards, that my decision was justified. In this choice I was conscious of myself.

This introspection confirms Michotte's results.[1] But this is not all; it confirms Ach's results also. Here is the introspection couched in terms of *Bewusstheiten*—something quite possible to do without in the least fabricating the gross facts of observation.

[1] It would have been a very easy matter to have confirmed Michotte in my study on choosing had my reagents not analyzed their experiences rather than resorting to Kundgabe.

At the outset I had the *Bewusstheit* that the upper numbers were large and that the lower numbers were small; then the *Bewusstheit* that I would add which was subsequently changed to the *Bewusstheit* that I would subtract and with this latter came the meaning that subtraction would be easier. I also had the *Bewusstheit* of determining to subtract, and that it was I who was doing the subtracting.

Here, however, is the same act of choosing, *analyzed.*

At the outset visual attention was fixated rigidly upon the three upper numbers; each number was rapidly and superficially perceived; none were definitely recognized or labelled as 2, 5, and 8. In this visual fixation there stood out vaguely the blackness and shape of the numbers with momentary pauses of line of regard upon a curve and a certain portion of an individual number. During this time attention was otherwise centered upon a visual-kinæsthetic consciousness of the musculature about the face, shoulders, and back of the neck. There then began to develop a consciousness that the upper numbers were large but this meaning matured only after the choice had taken place and during the act of introspecting. This awareness that the numbers were large, at the time consisted (1) of a slight tendency to retard eye-movement over the 8 and 5; retardation over the 8 was greater than over the 5; (2) it consisted of fairly intense strains in the throat as if some word were to be innervated. (Presumably the word 'large' would have been innervated at the time had this tendency developed further.)

Then visual line of regard shifted suddenly to the lower figures, with superficial perceptions of the 1, 3, and 5, again with no conscious recognitions. At this juncture the throat tensions mentioned above relaxed suddenly, accompanied by lessening of tensions in the muscles of the lower jaw. (No consciousness that the lower jaw was tense had previously developed.) These experiences were subsequently interpreted as meaning that the lower numbers were small. Visual attention then returned and lingered momentarily upon the figure 1; throat and jaw tensions continued to abate; these kinæsthetic experiences constituted the beginning stage of a consciousness that the lower number was smallest at its left-hand end but the meaning, 'smallest at its left-hand end,' matured after the decision had taken place and during the act of introspecting.

Then appeared the *Aufgabe* to decide, present in terms of rapid eye-movement from the lower to the upper row of numbers and in the extremely syncopated and fleeting auditory-vocal-motor imagery: 'Shall I add or subtract?' At this juncture attention broadened in its scope and included kinæsthetic sensations from the region of the forehead, tactual and kinæsthetic sensations from a slightly tensed facial expression, strains in the throat, visual perception of the numbers of the card, visual awareness of the space between these localized kinæstheses and the card, and the vocal-motor imagery just mentioned. In this latter imagery there was a momentary attentional emphasis upon the auditory quality of the vowel, 'I.' All of this was an interpretative period constituting an awareness of the self in the act of choosing. There was no elemental consciousness of self present.

The vocal-motor-auditory imagery just mentioned was very fleeting, yet while it was tapering off in consciousness there appeared the following: rapid shifts of visual attention to the lower right-hand 5, to the upper right-hand 8, then to the lower middle 3 and the upper middle 5; accompanying tensions developing about the throat and face. Then, with no intervening sensory and imaginal experiences and tendencies to interpret, visual attention shifted to the 1 at the left end of the

lower figure; the strains just mentioned suddenly abated. (During the act of introspecting, experiences prior to the visual fixation of the number 1 and its attending kinæstheses were interpreted as a tendency to add the numbers.) With the sudden release of facial strains and with the visual fixation of the figure 1 there developed, as was subsequently interpreted during introspection, a tendency to subtract. Just as visual attention centered itself upon the 1 there appeared in marginal consciousness a final awareness of the verbal imagery, 'Shall I add or subtract,' tapering off. At this juncture, with no intervening basal experiences and interpretative tendencies, there suddenly developed a diffuse and yet fairly intense 'jerk' of facial musculature with attending tensions in the throat, chest, and shoulders. With this 'jerk' came the clear-cut and very rapid vocal-motor-auditory imagery: 'I will subtract,' together with a rapid shift of visual attention from the figure 1 across the lower number toward the right. This rapid shift was the beginning stage of a consciousness later reported in terms of interpretation that the lower numbers were small. This interpretation matured and extended during the act of introspecting until it involved the meaning, 'subtraction would be easier.'

Before the verbal imagery, 'I will subtract,' had vanished from consciousness the act of subtracting had already begun in terms of eye-movement and shifts of visual attention, later in terms of definite vocal-motor-auditory imagery. Along with this act of subtracting, all bodily strains tended to disappear and before total relaxation had set in there developed numerous tendencies to summarize and interpret what had just taken place. The decision had already been made in terms of the muscular 'jerk' mentioned above and in terms of visual fixation. Notwithstanding this fact, during the interpretative periods which followed there matured the meanings: 'deliberate,' 'feeling of activity,' 'consciousness of the self.' The meaning, 'deliberate,' came from a rapid re-experiencing in terms of imagery the previous behavior of visual attention in tending to add before tending to subtract; the feeling of mental activity was an interpretation based upon the rate with which the verbal imagery, 'I will subtract,' entered consciousness, upon the marked distinctness of this imagery and upon the sudden development of tensions in different parts of the body. 'Consciousness of self' began to develop during the act of choosing as was described above; after the decision had taken place it matured but consisted exclusively of visualized facial kinæstheses, visual awareness of the space between these kinæstheses and the card, and visual awareness of the card. The mechanism of this interpretation was a sudden broadening of the span of attention. Consciousness of deciding, and of choosing for a serious motive followed from tendencies for the interpretations, 'consciousness of self' and 'feeling of activity,' to persist in the presence of the dwindling interpretations, 'deliberate.'

In none of these interpretations did the meanings mature until there developed strong vocal-motor tendencies to label them.

This introspection demonstrates the difference between analyzed and unanalyzed experience. It shows, further, that Ach's and Michotte's reagents were giving reports of unanalyzed experience. Since conclusions and summaries cannot be more analytical and penetrating than the data upon which they are based we can readily understand how Ach's

and Michotte's investigations failed to give us reliable introspective information about the volitional consciousness.

The introspection shows, further, that such experiences as Ach and Michotte thought were important to the volitional consciousness may appear after the act of choosing has been completed. This fact I pointed out in our original study. There is no reason to suppose, however, that in another instance these interpretations might not have appeared before the decision had taken place but even so they would have remained interpretations and secondary elaborations of basal sensory and imaginal contents. Only when a reagent is predisposed to interpret his basal experiences as meaning 'feeling of activity' and 'consciousness of the self' do such latter meanings develop.

I stated, secondly, that such unanalyzed experiences as *Bewusstheiten*, feelings of mental activity, impressions, doubts, consciousness of the self, *et cetera*, are interpretations from unrecognized sensory and imaginal contents. The truth of this statement should now be evident, for (1) it is possible to observe these interpretations, to analyze them and to tell when and where these meanings are being derived from the basal contents of sensation and image; (2) it is possible to experience these complexes and, in the absence of a proper analytical attitude, to fail in noting the sensory and imaginal ingredients of which these complexes are constituted. In the introspections given above the reagent did not recognize sensory and imaginal contents until he changed his mental 'set' from that of narrating meanings to that of giving analytical descriptions. In the absence of the appropriate analytical attitude details escape one's notice and experience appears only as a complex and as meaning.

Thirdly, I claimed that Ach's and Michotte's reagents had similar experiences in the volitional consciousness and in the act of choosing as did our reagents but that the former reported their experiences in the form of unanalyzed meanings, largely, while the latter reported their experiences in the analyzed form of sensation, image, and behavior of attention. As evidence and argument in favor of this claim I

point to the following considerations: (1) The introspections given above show that it is possible to describe the volitional consciousness unanalytically in terms of *Bewusstheiten*, feelings of activity and other equally as complex meanings without fabricating the gross facts of experience. (2) It was shown how weak and meager were the introspections of Ach's and Michotte's reagents by describing the volitional consciousness in terms of their language and meanings and by subsequently analyzing these meanings. We can exclude, therefore, as accurate descriptions any report of a *Bewusstheit*, feeling of activity, consciousness of the self, remark, attitude, recognition, affirmation, confusion, and the like until these meanings are analyzed into sensory and imaginal contents. (3) The fact that we all know when we are choosing and when we are experiencing a volitional consciousness is sufficient evidence that under all circumstances we are experiencing the same processes in the main. (4) I have adhered to Ach's and Michotte's definitions of *Bewusstheiten* and feelings of activity and have verified the existence of such processes. (5) My reagents, in choosing, experienced *Bewusstheiten*, feelings of activity, and consciousness of the self. That I do not mean by these processes what my critics mean by them does not follow either from the fact that I have analyzed them—something which Ach's and Michotte's reagents failed to do—or from the fact that I have used different words to describe them.

Lest our critic be unconvinced of the fifth point just mentioned let us turn to some of my reagents' original introspections on choosing. On page 9 is this statement from observer *B*: "These experiences were then followed by a short but sharp exhalation and an adjustment about the throat to say, 'Oh.'" The reagent did not say 'oh' nor did he have verbal imagery of 'oh'; it was nothing more than what he might have said had the experience developed further. Had this reagent reported: "There appeared a motor adjustment in the throat with the *Bewusstheit* of 'oh'" he would have been describing his experiences in equivalent language. The meaning, 'oh,' was present as

knowledge but this knowledge was not a feature of his original experience; it was an interpretation following from a particular muscular 'set' of the throat and lips. In the same introspection this reagent said: "I was conscious that I wanted to hear both selections." Ach would have said: "I had a *Bewusstheit* of wanting to hear both selections." Our reagent, however, went on and described this 'want' the contents of which were kinæstheses from eye-movements, pleasantness and visual images of phonograph records. 'Want' was the meaning of this complex of sensations and images; it was an interpretation. This reagent also reported a 'suspense' and had he not been sufficiently well trained the experience of suspense would have appeared *only* as meaning—complex contents simultaneously present as knowledge—and he would never have noticed, as he did notice, that the contents of this 'suspense' were muscular sensations about the shoulders, brows, mouth, and eyes together with the vocal-motor: "What am I to do?" When unanalyzed, consciousness of suspense is a *Bewusstheit* of suspense; when analyzed it is sensation and image.

On page 12 observer *A*, while looking at two alternative pictures, reported the vocal-motor-auditory imagery: "Very pretty; very characteristic; don't remember where." *A* knew the difference between unanalyzed and analyzed experience and frankly confessed it, for he went on to say that in terms of this verbal imagery he was indicating that he did not remember the exact location of the scene. The elaborated meaning, that it was the location of the scene which he could not remember, was put where it belonged, namely, in the realm of interpretation. *A* would have described his interpretation in equivalent language had he said: "I had such and such verbal imagery with the *Bewusstheit* that it was the location of the scene which I did not remember." He demonstrated more *Bewusstheiten* when, in the same introspection, he said: "The verbal characterization of the first scene *meant* (italics, author's) that it was pleasing and familiar; the characterization of the second scene *meant* to me that it was both pleasing and characteristic." Ach

tells us that a *Bewusstheit* is awareness of meaning. Here they are in our own introspections on choosing but placed by the reagents in the realm of *interpretation*, where such reports belong.

It is obvious that feelings of mental activity are meanings. An experience could not be labelled a feeling of activity if it were not a meaning. We can find numerous examples of such meanings—*bona fide* feelings of activity—in our reagents' introspections on choosing. A vigorous example is described by *J* on pages 18 and 19. On page 28 is another which was explicitly interpreted at the time as 'active'; the experience there described *meant* a feeling of activity; it also meant other things such as 'attitude of acceptance.'

Michotte's reagents confessed that they were able to designate 'feelings of activity' but were unable to analyze them. James said the same thing about the volitional consciousness in 1890! But this does not prove that 'feelings of activity' are unanalyzable meanings. That we can trust Michotte's introspections as valid and final is impossible in the face of his meager analyses of every other type of experience found in the volitional consciousness. On the other hand, in my investigation on choosing, feelings of activity were variously described and analyzed, for the very reason that their contents do vary from individual to individual. These feelings were reported as motor adjustments, tinglings, jerks, excitement, and attitudes. Why, in Michotte's or Ach's investigations, do we not find these variable interpretations? Because these experiences appeared to their reagents as unanalyzed meanings and an unanalyzed meaning is a stereotyped phenomenon. For example, if introspectors always failed to analyze their consciousness of a task we would be led to conclude that this experience is the same in all reagents under all circumstances. All we could say about it would be confined to the expression: "The experience meant 'task.'" As a matter of fact, however, 'consciousness of the task'—the same *meaning* wherever found—is made up of visual imagery in one case, a verbal process in another,

a strain in another, and so on. In the higher thought processes uniformity is found in functions, not in structures.[1]

My fourth point is demonstrated by the introspections in this article. Here the reagent was conscious only of feelings of mental activity and equally as complex meanings as long as he maintained an unanalytical attitude toward his experiences. The same demonstration can be made in any laboratory with any number of untrained observers.

My fifth point is demonstrated by the fact that the introspections in my experimental study on choosing were not teeming with unanalyzed meanings. With an analytical attitude a feeling of mental activity becomes a motor adjustment, a 'jerk,' 'excitement,' etc.; with a more analytical attitude these latter meanings become sensation and image. Any complete and detailed introspection demonstrates the principle that gross interpretations are reduced to a minimum when the reagent assumes an analytical attitude.

My sixth point should now be evident. We have seen that a given group of mental processes may lead in two directions: (1) It may mature to consciousness of meaning, passing over into interpretative periods before the reagent is aware that he is interpreting. The reagent then fails to recognize the sensory and imaginal components of these meanings and he reports unanalyzed experience such as did Ach's and Michotte's reagents. (2) These mental processes run their course in consciousness, rising to the focus of attention as sensations and images. The same meanings develop, however, but are found to be analyzable and to be interpretations. In the former instance the reagent re-

[1] I have already pointed out that unnecessary variations in terminology and in descriptions of the will have confused the problem and that these extreme differences have been due to various meanings which have accrued to a consciousness largely constituted of kinæsthesis. (PSYCHOL. REV., Vol. 27, 1920, 351–360.)

Miss Calkins has inferred from this (*Op. cit.*, 356, 357) that I identify the will with kinæsthetic sensation, an inference quite unwarranted, for I stated that the volitional consciousness is largely, not wholly kinæsthetic and that kinæsthesis turned out to be the only essential type of elemental consciousness in the act of choosing. Other types of consciousness as for example the visual, auditory, tactual, olfactory, and gustatory are present in the volitional consciousness but no one type is essential; one or a certain group does quite as well as another or a different group. See pages 7, 25 and 32, Univ. of Ore. Publs., Vol. 1, No. 2, 1920.

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ports *Bewusstheiten* and feelings of activity as unanalyzable because he fails to discover that he is interpreting; in the latter instance, because of a refined introspective technique, the reagent is able to report these experiences not only as complexes but as complexes whose exclusive constituents are sensation and image. He is aware that he is interpreting and can introspect not only upon the sensations and images but also upon his process of interpreting. Consequently he reports: "I was conscious of the self; this consciousness consisted of such and such images and sensations;" "These basal experiences *meant* to me a feeling of activity."

If a reagent fails to interpret a kinæsthetic complex as a feeling of activity it does not mean that he has omitted a component of the volitional consciousness; he has merely failed to *interpret* what his basal experiences might have meant to him. The will act or the choice does not depend upon the development of these meanings but upon the functioning of sensation and image. It is clear, then, why my results on choosing differ from the results of Ach and Michotte. We were describing the volitional consciousness in terms of its basal contents, not in terms of stereotyped meanings and variable, secondary elaborations of basal contents. In other words, Ach and Michotte described the volitional consciousness in terms of unanalyzed experience; we described the same consciousness in terms of analyzed experience. In so doing, the secondary meanings for which Miss Calkins pleads did not mature in all reagents until they had retrospected; in other instances these meanings did not appear at all. Yet the reagent was quite as well able to choose and to choose *vigorously*. The 'normality' of a choice accrued to intensity and complexity of basal sensory and imaginal contents, not to unanalyzed meanings.

It is of vital importance to the entire problem of unanalyzed *versus* analyzed experience to bear in mind that the act of interpreting does not depend upon non-sensory and non-imaginal contents.[1] We need no unique element or entity in consciousness, aside from sensation and image in

[1] Wheeler, 'The Development of Meaning,' *Op. cit.*, p. 231.

order to become conscious of feelings of activity, of willing, and of the self. The fact that the trained observer is able to analyze these experiences into their exclusive sensory contents does not leave it open to question that a unique element—consciousness of the self for example—has been overlooked and is lurking in the margin of attention undiscovered. The biologist does not find in a muscle a unique content, substance or element over and above the cells which constitute the muscle! The chemist does not find after he has analyzed a compound into its ingredients that the compound itself is sitting on the edge of the beaker looking at him! I ask Miss Calkins if in respect to self-consciousness she is not the one who has left something undiscovered in the margin of attention? This something is an act *of interpreting*, which, unanalyzed, gives to her consciousness of the self the very thing which she accuses me of overlooking.

That "consciousness of the self," like the Bewusstheit and the feeling of activity, is a complex meaning, varying in content but quite analyzable into sensory and imaginal processes, is shown by the following introspections. These descriptions are samples obtained by the writer from time to time in the laboratory with the aid of an assistant who read the instructions to him. Such processes develop in a multitude of situations: in the laboratory under controlled conditions; in the solitude of one's study; in repose during a period of relaxation or what not.

Instructions: "Let your attention shift focally to a consciousness of self and at once write your introspection."

1. This was a mild form of self-consciousness. At the outset there was a sudden welling into focal consciousness of a definite and widespread blend of kinæsthetic and tactual sensation from the regions of the back of the neck, the facial muscles, forehead, and scalp. These experiences were not well localized at first but as they reached the focus of attention they became definitely localized; part of this latter process consisted of visualizations of the musculature involved. Then attention centered itself almost wholly upon the facial muscles and with this tendency there developed fleeting but momentary, vivid, visual imagery of the circular outline of the face as if trying to see the face. This imagery was localized within a few inches of the eyes. Then followed an interpretative period; this consisted of slight tendencies to relax, localized in the arms, shoulders, and abdominal muscles; these kinæsthetic experiences constituted a feeling of familiarity; attention suddenly shifted to the

face, then to the throat as if reviewing the processes just experienced. The feeling of familiarity and interpretative period ended in a vocal-motor-auditory image: 'myself.'

Instructions: "Think of yourself in contra-distinction to another person." *O* was sitting at the typewriter and wrote his introspection immediately after the consciousness of self developed.

2. This was a vigorous consciousness of self. First there appeared marked kinæsthetic and tactual, resident sensations from the face, about the mouth, forehead, back of the neck, and shoulders all constituting a blended experience at the outset with the sensations from one region quite as clear and intense as sensations from any other region save for the forehead and temples where the tactual element of pressure stood out more prominently. Then, consciousness of self suddenly increased in its intensity; this increase consisted of the sudden appearance of an organic-kinæsthetic consciousness of the abdominal region, particularly at the belt. On further analysis sensations from the belt-region turned out to be pressure and the organic sensations turned out to be due to muscular movements of breathing. Along with these latter experiences there developed a tenseness in the throat, strong tendencies to visualize and more definitely localize the musculature involved thus far in the reaction.

Then came visual imagery of 'myself' projected off about five feet away and at a sharp reading angle to the left. In this visual imagery were one side of the face, the hair and one shoulder, in which latter region there momentarily stood out the mottled-grey of the coat being worn at the time. Here tightness in the throat increased; there developed incipient vocal-motor imagery of the word, 'self,' and the auditory (no vocal-motor element) of the word, 'Ray.' The instant the latter image appeared there was a momentary rise in intensity of kinæsthetic processes from various regions of the upper body—a diffuse and widespread shift of attention to kinæsthetic qualities although these sensations were not localized. These latter experiences constituted an interpretative tendency that the experiences as previously described meant consciousness of the self.

In the more vigorous forms of self-consciousness the vigor consists of more widespread and intense cutaneous and kinæsthetic sensations with more definite tendencies to visualize the regions where these experiences are developing. Verbal processes are more profuse and varied. Organic sensations from the regions of the stomach, diaphragm; circulatory sensations from blood pressure beneath the skin of the temples; temperature sensations from the skin of the forehead or cheeks, and like experiences, enrich the consciousness and play a more dominant rôle in the more vigorous cases. In no instance, however, is there to be found any feature of self-consciousness which is not sensation and image.

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At times I experience a consciousness of self which is nothing other than a fleeting visual image of my own face. With a characteristic fixation of visual line of regard upon certain facial features there at once follows a momentary motor response, very diffuse and widespread, constituting a feeling of familiarity, perhaps, or at any rate functioning as the recognition of the image as 'I.' There is no non-sensory element present. 'I' is meaning, here, just as 'task' is meaning when one is conscious of an *Aufgabe*.

It has been my experience, therefore, that a typical self-consciousness develops somewhat as follows: (1) Tactual and kinæsthetic sensations from the regions of the throat, face, scalp, back of the neck, and perhaps shoulders and trunk; (2) a behavior of attention in which these experiences are 'cognized' as a 'total experience.' This cognizing does not introduce non-sensory elements; it consists merely of a redistribution and a changed emphasis among these first contents as they now persist, together with the appearance and functioning of visual or vocal-motor-auditory imagery or both. With the appearance of these supplements the kinæsthetic processes usually become more intense. In some cases tendencies to visualize are at a minimum and are confined almost wholly to schematic fixations of one's line of regard upon different regions of the body without any visualization of musculature. (3) There is a second redistribution of sensory and imaginal contents in which vocal-motor-auditory imagery plays a more dominant rôle. Here there may be a rapid, attenuated review (perhaps kinæsthetic, perhaps visual) of the entire motor adjustment. With each appearance of vocal-motor-auditory imagery there is a definite increase in intensity of the motor adjustment while localization of the kinæstheses may be less distinct. This period constitutes a feeling of familiarity and a definite recognition of the experience as consciousness of self. The meaning, self, has now become fully explicit. (4) The process of interpretation which began with stage (2) reaches its final stage in a recognition of the fact that 'consciousness of the self' has developed. This is a verification process

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adding nothing to the consciousness of self but which oftentimes runs to elaborate reviews of organic-kinæsthetic experiences and to trains of visual or verbal imagery.

The important facts about the self thus seem to be as follows:

1. The contents of self-consciousness are exclusively sensory and imaginal.

2. Self-consciousness is context, meaning, synthesis, whose uniqueness lies in function and not in content.

3. A full-fledged consciousness of self is an act of interpretation (as is any meaning) which passes through four fairly distinguishable stages, each merging into the other. It is an inherently flowing consciousness whose meaning is defined not by specific contents but by the rôle or setting of that consciousness. In no one of these four stages is there present any content which is not totally reducible to sensation and image.

4. Unless the reagent is sufficiently well trained in introspection he will be unable to discover these stages and will erroneously mistake interpretation for basal experience, whereupon he is led to accept unanalyzed experience as elemental. He reacts to meanings and mistakes the meanings for mental elements.

5. These considerations do not explain away the self. They merely aim to give to it and to consciousness of self a scientific description.[1]

[1] Miss Calkins seems to feel that 'self-questioning' on the part of certain of my reagents in the study on choosing points to her type of 'self-consciousness.' (*Op. cit.*, 368.) Need Miss Calkins be reminded that 'self-questioning' by the reagents' own confessions was a matter of interpretation—a phenomenon of meaning—derived from no other processes than basal sensory experiences. The contents of any such consciousness of self were vocal-motor-auditory images of 'I,' head and trunk kinæstheses and visual imagery, perhaps, of the musculature or bodily parts involved.

Miss Calkins also asserts that the frequent use of the word, 'I,' at the termination of the decisions indicates a self-consciousness which we neglected to take into consideration in our discussion of the volitional consciousness. Such processes were in part the contents of a sensory and imaginal self-consciousness or they had to do with the act of designating to the experimenter the alternative already chosen. In other instances they were contents of a reflective or interpretative consciousness by means of which the reagent summed up what he had already done.

The dangers which befall the psychological experimenter when he fails to distinguish between unanalyzed and analyzed experience become all the more serious in the light of the following facts:

1. Tendencies to interpret from basal sensory and imaginal experiences take place with lightning-like rapidity and unless the reagent is sufficiently trained to discover that he is interpreting grossly instead of analytically, he will never be the wiser. He will be led as was Miss Calkins to criticize introspections which carry the analysis beyond him. He will be led further to localize the fault in the wrong place, namely in something that he thinks the introspector has failed to discover. He will mistake his own unanalyzed interpretation for a basal experience which his opponent has failed to report.

2. It is possible to experience a vast complex of basal contents such as visual imagery and kinæsthetic sensations, to react to these contents, *i.e.*, to recognize their meaning and immediately afterward to mistake the recognition as the only process which had to do with the meaning. As a matter of fact the meaning never develops in the absence of these basal contents.

3. The purpose of a scientific investigation, where introspection is involved, is to describe the elemental constituent of experience, not to be content with statements of meaning. The basal experiences are the important things to discover. In the act of choosing, such experiences as 'jerks,' 'adjustments,' 'friendliness,' 'feelings of mental activity,' 'consciousness of the self,' 'attitudes of acceptance' are one and all meanings. None of them are basal experiences. They are individual elaborations of basal experience. I found it possible to reduce these variable interpretations to a common ground—sensory and imaginal content. I could not have summarized the facts of the volitional consciousness by having recourse to these meanings for they were variable from individual to individual; they were intellectual processes intruding upon and giving meaning to basal contents. Such interpretations have no more to do with the act of choosing *per se* than does consciousness of the fact that we see red have anything to do with the act of seeing red.

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4. It is too easy to interpret *from* the basal experiences of sensation and image and to labor with the interpretation rather than with the original experience. This is what Miss Calkins, Ach, and Michotte have done with respect to the volitional consciousness. I urge that my critic meet me fairly and squarely, not with verbal contradictions, but with such introspective data as will show the difference between analyzed and unanalyzed experience.

In conclusion, therefore, I reiterate my former assertion that in the volitional consciousness there are to be found no feeling of mental activity and no consciousness of the self which are not exclusively constituted of sensation and image. I point out further that I never denied the existence of either one of these experiences; I merely claimed that they were neither elemental nor unanalyzable and that they belonged to 'interpretative periods.'

My critics may choose either to deny the truth of introspective descriptions in which case they are denying the facts of observation or else admit that because of a superficial introspective technique they have failed to analyze their own mental processes.

WHAT CAN PSYCHOLOGY CONTRIBUTE TO OUR KNOWLEDGE OF THE MECHANISM OF MENTAL DISORDER? WITH SOME COMMENTS ON THE TERM "DEPRESSION" [1]

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The physician dealing with symptoms of disease seeks their explanation in the light of principles which he has acquired in the various disciplines preparatory to clinical work. He seeks to understand the disordered action of the heart according to the principles which he has learned in physiology, biochemistry, bacteriology, and pathology; in the case of the disordered action of the individual, he naturally summons, in addition, the help of that discipline which is especially concerned in establishing the principles underlying human behavior, the science of psychology.

The physiologist and the psychologist may be of help in clinical problems in one of several ways; they may have accurate observations on controlled material that may throw light on the mechanism of the disordered reactions; they may suggest methods of approach to the solution of the problems; they may even adopt a problem as their own, and work at it under the accurately controlled conditions of the laboratory.

The physiologist is apt to find the complexity of the clinical situation unsatisfactory, as he is seldom able to control the conditions with the accuracy he desires; still he may find the clinical material of interest, it may enable him to check results gained under abstract laboratory conditions, it may even suggest to him profitable lines of future work. So the psychologist may find rather unwieldy the crude stuff of life as it has to be dealt with in the out-patient department and the wards, in the home and in the workshop; but at the same

[1] Presented before the Meeting of the American Psychological Association, Princeton, December 29, 1921.

time he may find the material of value for his own special work, and may even be stimulated to take up problems which hitherto have been somewhat neglected. It was natural that in the development of accurate laboratory procedure problems of perceptual experience and simple reactions should have at first to a large extent monopolized the field; the problems of the emotional life lend themselves less readily to laboratory investigation. In regard to problems involving fatigue, the conditions of learning, the factors modifying the grasp, elaboration and retention of impressions, a much greater body of accurate information has been available than in regard to the emotional life. Much of the raw material of life with which the physician has to deal is intimately connected with the emotional life of his patients, and he eagerly welcomes the coöperation of the psychologist in the investigation of this field.

An example of the material which the psychiatrist has to deal with is furnished by the following case, the discussion of which may peradventure be of interest to the psychologist. The case is chosen because in the center of the clinical picture there stand out the emotional or affective factors, and the psychiatrist finds it no easy task to reduce these to any simpler component forces.

The patient, a woman of forty-three, was admitted to the Boston Psychopathic Hospital April 9, 1921. She had made a half-hearted attempt to cut her throat, but desisted.

The patient came from a stock free from evidence of nervous instability. She, herself, had lived an apparently uneventful life. After a somewhat delicate childhood, she became a fairly robust adult. She worked as a teacher for some time; married at twenty-six; had four children. Externally her life appeared to be very satisfactory and happy.

At the age of twenty-five she lost her father, and for a few days was so depressed that the doctor was called in. At the age of forty-one she lost her mother, and here again the depression seemed to be somewhat extreme. She seemed to lose ambition and could not do her housework for a month or so.

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About March 1, 1921, the patient began to be depressed. She later asked her husband about a letter which he had written before marriage in relation to some affair with a woman. She seemed to realize for the first time what it meant, and stated that she could never forgive him. During the following month the patient talked frequently about sexual matters, and there was more sexual desire than usual. She felt that she couldn't work, and that her love for her husband and children had died. She thought that she would never get well.

When admitted to the hospital the patient complained of feeling sad, and this was in keeping with her general appearance. She was despondent about recovery, felt that she would be put away in some asylum. The patient showed no special slowness in her conversation, and she was able to carry out the ordinary tests of intelligence in a satisfactory way. The physical examination disclosed nothing of importance save general undernutrition.

In regard to such a case the physician has little difficulty in describing and classifying; the difficulty is in understanding the underlying mechanisms, in tracing the chain of cause and effect, in specifying exactly where the weak link is, in formulating accurately the nature of the weakness.

The condition can obviously be referred to as one of morbid depression. The patient's grasp of the outside world shows little distortion, and certainly no evidence of hallucinations or delusions. As to the ability to receive impressions and elaborate them and come to logical conclusions, and the ability to utilize memories, one may say that the intellectual processes are intact. The disorder is chiefly in the feeling life of the individual, on which ground one may use the term 'affective psychosis.' In the old medical classification the disorder would be 'melancholia'; in the more recent classification some might call it a 'manic-depressive psychosis (depressed type).' This latter denotation carries with it the further significance of a probably favorable outcome with a tendency, however, for the patient to have from time to time similar attacks, or attacks of converse nature with elation

and over-activity. The history of the patient fits in with this conception; she has, as a matter of fact, on two occasions had brief periods of depression, the one after the death of her father, the second after the death of her mother. These episodes seemed to be more than the ordinary reaction to bereavement, for on both occasions a physician was called in to examine the patient.

There is a tendency to refer such forms of sickness to a constitutional basis, that is, to assume that the patient is so endowed congenitally that, from time to time, her feeling-life is upset, either with no obvious precipitating cause or to a degree which is considered disproportionate to the precipitating cause. Can we formulate more precisely this special form of instability? The physician looks for other conditions of emotional disorder, to see if they may throw light on the present case. He is familiar with some organic diseases, which involve special portions of the brain (basal ganglia) and lead to different types of disorder of emotional expression (explosive laughter and crying in pseudo-bulbar paralysis and in multiple sclerosis, fixity of emotional expression in Parkinson's disease). Other conditions associated with emotional disorder are due to toxic factors in relation to special glands; in disturbed activity of the thyroid gland one may meet conditions of overactivity and restlessness and emotional instability on the one hand, and on the other hand conditions of depression and under-activity. There are also many observations of disorders involving other glands of internal secretion where the mood or temperament of the individual seems to be correlated with the disturbed balance of the glandular activity, although we are not yet in a position to formulate temperament in terms of the physiology of these glands.

A latent infection may cause us to feel depressed and in the complex equilibrium of forces, which corresponds to the depressed state, memories and thoughts tend to come to the surface which under other conditions are repressed or not in evidence. One has not the same energy for attacking tasks; one foresees a gloomy outcome to one's efforts, one thinks of

past failures. In one's general adaptation to the outer world and in one's reconstruction of past experiences there is not that aggressive and confident self-assertion, with elimination of all contradictory indications, which goes with the opposite state of equilibrium, with the balance of the self-confident or happy or elated mood. In the average condition of comfortable and active existence, the assertive, agressive, efficient factors have the upper hand and there is a more or less automatic repression of disturbing memories and of indications of present weakness. This is the equilibrium of health, when energy is being employed in a well-balanced manner and when satisfaction of various kinds is supplied by the environment. When a depression supervenes with all its attendant gloomy thoughts we have to analyze the change and see what it is which has altered the total balance of the individual's condition.

In relation to our case with the recurrent periods of emotional upset, some of the above considerations do not carry one very far, for we find neither unequivocal evidence of any structural damage nor of any toxic process nor of any disturbance of glandular function. We are, therefore, thrown back on a more or less descriptive formulation of the disorder. We may consider that the patient is specially liable in view of her constitution to attacks of depressive coloring, and in this connection we keep in mind the fact that there are some individuals, not subject to marked episodic variations of mood, whose whole outlook on life has a tendency to a definite depressive coloring, while others show throughout their lives the opposite temperament.

The special reactions of our patient seem to be closely related to the problems of the affective life in general, and one turns with interest to see what normal psychology has established with regard to the laws of the feeling-life of the individual. One finds much with regard to sensation, perception, discrimination, time of reaction, and with regard to the influence of experience on the intellectual processes and on the motor habits. With regard to the feeling-life of the individual one has a body of information with regard to the

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more fundamental reactions, such as fear or anger, and also suggestions as to the combinations of these simple reactions into more complex patterns. With regard, however, to such a familiar reaction as depression one finds comparatively little description or analysis.

So far, therefore, the psychiatrist in relation to the actual case of depression under discussion finds no useful formula already prepared by the biochemist, the physiologist, or the psychologist. His own review of the case has disclosed a familiar type of reaction, but has furnished no key to make the reaction intelligible. The review, however, has been rather superficial and one has been too easily satisfied with stock descriptive terms; it has been more or less assumed that 'depression' has a well-defined connotation and is an adequate expression for the feeling-tone of the patient and whatever that depends on.

The term depression like so many other psychological terms tends to be considered a definite entity, which in virtue of its independent existence becomes the cause of further phenomena. It is prudent to translate the abstract term back into the complex situation for which it stands, and in trying to understand sequences it may be necessary to consider the total biological situation.

In the light of these considerations it may be well to come back to our patient. When questioned about her mood (December, 1921) she answers that she is 'miserable.' "I feel pretty bad for the children—it seems as if I were being punished for something."

These statements suggest that the mood of the patient may be related to definite topics. She feels distressed in relation to the family situation; she seems to have an uneasy feeling that the present situation may be related to previous guilt.

The patient in the above utterances may have made a false connection; she may attribute her depressed feeling to a certain situation, while the real origin of the feeling may lie elsewhere. So in physics one may misjudge the source of a visual image, and have to make allowance for the influence of the refracting medium.

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This does not mean that one cannot get much of value from the introspective account of the patient, nor that we have to limit ourselves to a 'behavioristic' formulation of the situation. The physician deals with the account of the patient as he does in regard to any other form of sickness. As the patient tells him of his pains, so he tells him of his feelings, his beliefs, his impulses. It is true that each datum needs to be controlled by others, and allowance made for sources of misinterpretation. The patient may complain of pain in the toes when the leg has been amputated; he may claim memory of experiences which have been non-existent.

The psychiatrist in his spare time may indulge in epistemological considerations and maintain a psychological solipsism; he may appear to be the only solid certainty in a world of possible illusion, and may consider that thoughts and feelings of others are unattainable objects of knowledge; for his practical tasks he accepts language, not merely as conveying information with regard to external relationships, but as a medium for putting us directly in touch with the inner life of our fellows. He deals with the reactions of sick people; he studies their behavior; he listens to their statements; he seeks to grasp their outlook on life and to enter into their feelings. He may observe movements incorrectly; he may accept statements made dishonestly; he may misinterpret the attitude of the patient to the outside world; he may realize most imperfectly how the patient feels. Making allowance, however, for all his mistakes he finds that as time goes on, with increasing experience, his endeavor to study the patient from all these points of view brings clearness to many a situation, is of great practical use and enables him to help the patient.

To return again to the patient with whom we started. Discarding the stereotyped formulæ into which most cases tend to be squeezed, and utilizing the patient's own account of the matter in the same prudent way in which we utilize the statements of our fellows for the ordinary purposes of social life, we find the situation not so simple as the textbook descriptions present it, but more intelligible than in its previous abstract presentation.

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The first change remembered by the patient was "suddenly this feeling came over me that something was wrong, this feeling that everything wasn't all right." A short time later "I couldn't seem to be contented to stay up; I got weak." Then she "tried to pray and to get back God's Love."

In discussing the sequence of events, the patient, referring to her unworthiness, said that she had 'spoken quarrelsome to her mother-in-law before the children.' The content of her talk had been complaints about her husband, especially about his having been out late the night before. She even went so far as to talk of leaving her husband. All this out of a clear sky, for they had apparently been a congenial couple. As a matter of fact the husband had been out late the night previous to her complaint to her mother-in-law, and she had sat up reading a religious book. Quite suddenly his lateness had struck her, she began to worry and when he returned she was annoyed, 'I jawed and jawed him'; and when he retired to bed she spent two hours in her living room 'thinking just ugly, real ugly feelings; it seemed as if my whole nature changed then and there.' This ugly feeling persisted until the visit of the mother-in-law upon the following day, when the patient made quite unjustifiable charges against the husband, saying he did not feed nor clothe the family. The husband admitted that he had been somewhat at fault in indulging in card-playing, and there was an apparent reconciliation. After two weeks, however, "I began to get this feeling, the beginning of the breakdown, a feeling beyond description, that something was going to happen to me, something had come over me that I could not shake off." She then turned to God for help and later recalled that she had not made her first communion right, and assumed that her present state was due to this mistake. In trying to explain the reason for her trouble she said, "You see I did not get the love of God first and, therefore, did not get the love of man." She amended this to explain that she had got the love of man but "I did not reflect the love back; I didn't feel as I should have" (*i.e.*, sexually almost anæsthetic). In relation to this topic one may mention again

the fact that a week before she came to the hospital she began to talk to her husband about the past episode in his life, about some intimacy with a woman which he had once confessed to her, but of which she now for the first time began to realize the significance, according to her account. During this period she had marked sex desire, which was quite unusual with her, for during her married life she had been practically anæsthetic. Shortly before the onset of the first symptoms she had moved into a bungalow with her family: "It came home to me, a nice little place for a couple to live. Perhaps I began to think I had not had much out of life, it seemed as if we were starting life different." After the removal she looked forward to having all her own furnishings, and to getting rid of all her mother's furniture with which she had begun her married life, when her mother lived with her. When asked whether this change to the bungalow might have had an influence on her emotional life she said, "I think it may have. No! I don't; it was God's own time. You can't get past God." A further factor in the case may be mentioned. Before she married she had had several mild love affairs. As soon as a suitor proposed she was through with that affair. One affair had been more serious and had been broken off through some misunderstanding. Throughout her married life occasionally she would hark back to this affair, and day-dream about it having had another ending.

With these data one feels less inclined to look upon the "depression," or rather the patient's complicated play of mood, as an independent change coming on through some subtle and unknown changes in the physiological economy of the patient. Nor does it seem to be merely the expression of some vague constitutional lack of stability. It seems rather to be merely one aspect of a very complicated biological situation. In that biological situation, there were present unsatisfied cravings, vague yearnings, organic desires, repressed fantasies, stifled irritations, strivings towards religious solutions. With all this, the case is liable to be looked upon as a *Simple Depression*, whereas it seems com-

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plex enough, and the term depression fails to do justice to the chromatic richness of the inner experience of the patient. The tendency to squeeze such cases into the customary simple and rigid formulæ and to characterize them with such inadequate terms as 'depression' is largely due to the lack of a sufficiently rich and accurate terminology for describing the emotional aspect of human experience. The student coming from the psychological laboratory to practical applications, has had an opportunity of studying with great accuracy of detail the factors which modify perceptual experience, but has had little opportunity of analyzing the complex emotional reactions involved in the stresses and strains of real life. Pear[1] has recently shown how inadequate psychological language is for dealing with kinæsthetic data and how this poverty of language has tended to depreciate the value of kinæsthetic experiences and of manipulative expertness, and has given a false perspective to the values of different human activities. There has been perhaps a similar lack of systematic and accurate observation of emotional variations in human experience, and the student comes to his psychopathology poorly prepared for an accurate analysis and description of the emotional life of his patient. Equipped with knowledge of a few standard types, he then discards as largely irrelevant the rich complexity of the actual experiences he is studying, and tries with the standardized residual to formulate a working conception of the nature of the disorder. Perhaps in arriving at his polished and systematic formula, he has thrown away the vitamines contained in the irregular and richly variegated and individual coat of human experience.

[1] Pear, T. H., 'The Intellectual Respectability of Muscular Skill,' *Brit. J. of Psychol.*, General Section, October, 1921.

THE EFFECT OF VARIATIONS OF THE INTENSITY OF THE ILLUMINATION OF THE PERIMETER ARM ON THE DETERMINATION OF THE COLOR FIELDS

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Field study is coming to have a very important place in office and clinic work. By field study is meant the determination of the limits of light and color sensitivity; the mapping of scotomata and the blind spot; determinations of sensitivity and of disturbances in the distribution of sensitivity; etc. It is perhaps the most sensitive and valuable means at the disposal of the medical profession for the diagnosis and the study of the advance and recession of many pathological conditions of the eye, of the sinuses, and of the brain and its conducting paths. For the successful development of this work there is great need of the coöperation of the laboratory worker with skill and experience in devising, standardizing and applying functional tests.

The importance of the functional test in the medical work on the eye seems to be generally conceded. All seem agreed that in the diagnosis and the checking up of the advance and recession of pathological and other disturbing conditions the functional test is the most sensitive that has yet been proposed. Difficulty has been encountered, however, in applying it with the sensitivity and precision that is needed, particularly in office and clinic work. In some fields great advancement has yet to be made in delicacy and sensitivity; in others the more important need is greater precision and reliability. In all fields a more careful scrutiny of the functions affected is needed. Many attempts to apply functional testing have failed because the wrong function was selected to be tested. At one time it was held that a difference in the effect of good and bad lighting on the eye

could not be found by test merely because the eye functions most affected by bad lighting conditions were not tested. And today the acuity test is rendered needlessly insensitive, partly because the test is not applied under the most sensitive conditions obtainable, but mainly because the aspects of acuity most affected by ocular defects and special and disturbing conditions of work—namely, speed and power to sustain, are not included in the conventional method of testing acuity. In the pathological field we have yet to find out what are the most important functions to be tested.

The theory and practice of clinic field taking for color seems up to the present time to have been based on the following assumptions: (1) The far periphery of the retina is blind to color, that is, there are definite limits or boundaries of the sensitivity to colored light. These may be called the *actual* limits of sensitivity. The limits as determined, the apparent limits, may or may not agree with the actual limits depending upon the conditions under which the work is done. (2) The factors affecting the apparent limits when the fields are taken with pigment colors are the size and coefficient of reflection of the stimuli employed. A contraction of the fields taken under standard conditions of control for these two factors means then an actual change in the distribution of sensitivities. (3) Sensitivity decreases from the center towards the periphery of the retina in a fairly regular manner in any given meridian and the rate of decrease for the same color is approximately uniform from meridian to meridian over a considerable range near the limits of sensitivity. Irregularity of contraction, resulting in a change in the shape of the field, is not therefore to be expected to any considerable extent as a phenomenon of the normal eye. And (4) the field of sensitivity is wider for blue than for red. The limits of the fields should not then be expected to interlace or to reverse their order of ranking as to breadth without some pathologic or other disturbance in the distribution of sensitivities.

A study of the distribution of sensitivities in the peripheral retina and of the factors which affect the apparent limits of sensitivity shows that these assumptions are not correct.

The far periphery of the retina is not blind to blue and red, nor is the sensitivity in this part of the retina greater for blue than for red (at some points it is greater for blue, at others for red); the decrease of sensitivity from the center towards the periphery of the retina is very irregular in any given meridian and the rate of decrease is very different from point to point in the different meridians; the apparent limits of sensitivity are a function not of two but several variables; all of the field changes noted above as pathologic may occur and do occur as the result of nothing more serious than a defective control of the variable factors which affect the sensitivity of the peripheral retina to color.

The variable factors which affect the sensitivity of the peripheral retina to color are, so far as we have been able to discover, the wave-length and purity of the stimulus, the intensity of the stimulus and the visual angle, length of exposure of the eye, the method of exposure (moving or stationary stimulus), accuracy and steadiness of fixation, the intensity of the general illumination of the retina and its state of adaptation, breadth of pupil, and the brightness of the preëxposure and of the background or surrounding field. The most important of these factors, from the standpoint of the work of the office and clinic, are perhaps the intensity of the stimulus and the precision of its control, the brightness of the preëxposure and of the surrounding field, the intensity of the general illumination and the accuracy and steadiness of fixation. Some of these factors have been discussed in previous papers; some will be discussed in later papers. The present paper is an additional study of the effect of variations of the intensity of the stimulus on the determination of the color fields. When the color fields are determined with pigment stimuli of a given coefficient of reflection, the intensity of the stimulus light is of course dependent upon the amount of light received on the perimeter arm.

In a discussion of the factors influencing the determination of the color fields, intensity may be considered as typical. That is, whatever is true of the effect of intensity should be true also in a general way of the effect of any other uniformly

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acting factor which produces large variations in the breadth of the color fields. The common factor underlying the effect of any condition of work on the size and shape of the color fields and their order of ranking as to breadth is the type of distribution of sensitivity in the peripheral retina. It is this, for example, which determines how much contraction or shrinkage of the field shall take place with a given value of factor and whether shrinkage shall be accompanied by a change in the shape of the field or a reversal in the order of ranking of the fields as to breadth.

The Intensity of the Stimulus.—By a sufficiently wide variation of this factor alone, the fields of color sensitivity may be made to have almost any breadth in the field of vision, to differ radically in shape, and even to change or reverse their order of ranking as to breadth,—changes of a type and magnitude to which the clinician might ascribe a grave and important pathologic significance. For example, with very high intensities of stimulus the limits of red, yellow and blue are coincident with the limits of white light vision. Green cannot be made to have so wide an extent. With stimuli of medium intensity and of the relative energies found in the prismatic spectrum of a Nernst filament, for example, the limits are concentric and in the order from widest to narrowest of red, yellow, blue and green. With stimuli of medium intensities of equal energy (equal physical intensities) the limits of red, yellow and blue interlace or crisscross. The limits for green again are narrower. The limits for pigment stimuli may be either interlacing or concentric in the order from widest to narrowest of red, blue and green; or blue, red and green, depending upon the pigments used and the intensity of light falling on the perimeter arm. It seems only fair to conclude, therefore, that the conventional clinic rating of the limits from widest to narrowest in the order of blue, red and green is a function of the relative and absolute intensities of the stimuli employed as well as of the actual distribution of sensitivities. A change in the intensity of the stimulus not only changes the limits but, because of the irregular distribution of sensitivities in the different meridians,

causes a marked change in the shape of the fields; and because of changes in the ratio of sensitivity to the different colors in the same meridians, a change in their order of ranking as to breadth. Without great precision in the control of intensity, it is obvious that reproducibility of result cannot be obtained and little significance can be attached to extent or shape of field, to order of ranking as to breadth of field or to any variations from time to time or from person to person in these important features.

The effect of changes in the intensity of the stimulus both on the extent and shape of the color fields varies with the order of magnitude of intensity employed. For medium and low intensities the effect of a given amount of change is very much greater than for high intensities. This is an obvious corollary of the type of distribution of sensitivities found in the peripheral retina. That is, in passing from the center towards the periphery the decrease in sensitivity is comparatively slow and gradual in the paracentral retina; it is much faster and more abrupt in the mid-periphery; and very abrupt in the far periphery. It requires, therefore, comparatively large changes of intensity of stimuli of high intensity which carry the limits of sensitivity into the far periphery, to produce a significant change in the limits; not so great a change in the stimuli of medium intensity; and a still smaller change in stimuli of low intensity. This effect varies greatly, however, for the same color in the different meridians and for different colors in the same meridians. For the stimuli of medium and low intensities used in the office and clinic, the effect of change of intensity is very marked indeed both on the extent and shape of the fields of sensitivity.

We have stated that the order in which the fields may be found to occur, ranked with regard to breadth, depends both upon the actual distribution of sensitivities and upon the relative and absolute intensities of the stimuli employed. That it depends upon the relative intensities should be more or less obvious, provided of course that the relative differences of intensity are great enough to overcome or reverse the

actual differences in sensitivity. In support of the statement that it depends also on the absolute intensity, maps will be shown in which for the same eye and a constant ratio of intensity of stimuli, the limits for red and blue in some cases interlace or crisscross; in others they are concentric or nearly so—the limits for blue lying outside of the limits for red; or conversely the limits for red lying outside of the limits for blue—the difference in result depending solely upon the absolute intensities of the stimuli employed. The determinations were made with the Hering standard pigment papers presented to the eye by means of the perimeter[1] described in a preceding paper, with the preëxposure and surrounding field of the brightness of the color, the only variable being the intensity of the illumination of the perimeter arm.

The Chromatic Thresholds of Sensation from Center to Periphery of the Retina.—As we have already intimated, the effect of such factors as intensity on the extent and shape of the color fields and their order of ranking as to breadth can be understood better when a thorough knowledge is had of the actual distribution of sensitivities from point to point from center to periphery of the retina. In order to show this distribution we have made determinations[2] of the threshold of sensation in energy terms using spectrum lights and proper conditions of control, for the different colors at near-lying points from center to periphery of the retina. A graphic representation of the results of these determinations for two meridians, the temporal and nasal, is given in Charts 1–4, "Chromatic Thresholds of Sensation from Center to Periphery of the Retina and their Bearing on Color Theory," PSYCHOL. REV., 1919, **26**, 28–32. The following points of importance to this paper may be noted from these charts.

1. The great range of sensitivity from the center to the extreme periphery of the retina. For example, in the nasal meridian the sensitivity to red is 900 times as great at the center as at the extreme periphery of the retina; for yellow 1,650, for blue 3,280 and for green 3,380 times as great.

[1] *J. of Exp. Psychol.*, 1922, **5**, 46–67.

[2] The stimuli used were red (670 m μ); yellow (581 m μ); green (522 m μ); and blue (468 m μ).

2. The great irregularity of decrease of sensitivity which is found in passing from the center to the periphery of the retina. This irregularity moreover differs greatly in the different meridians. From these irregularities it is obvious why the shape as well as the extent of the fields of sensitivity changes with the change of intensity of the stimulus light. That is, depending upon the different rates of decrease of sensitivity in the different meridians, a given increase or decrease of intensity of the stimulus light causes respectively different amounts of extension or contraction of the fields in these meridians. The result is of course a change of shape of the field proportionate to the amount of irregularity of distribution of sensitivity in the different meridians.

A further obvious corollary of irregularity of distribution of sensitivity in the different meridians is the interlacing or crisscrossing of the limits when the stimuli are so graded in intensity as to give the limits for all of the colors approximately the same degree of excentricity. That is, a condition of coincident or concentric limits would presuppose regularity of distribution of sensitivity from meridian to meridian; irregularity inevitably leads to an interlacing or crisscrossing when the conditions under which the determinations are made are such that the fields have approximately the same breadth. A frequent crisscrossing of the limits occurs for red, yellow and blue when stimuli of medium intensity and of anywhere near equal energies are used.[1]

3. A third point is the great irregularity in the ratio of sensitivity to the different colors which is found in the same meridians in passing from the center to the periphery of the retina. At some points, for example, the sensitivity to red is greater than the sensitivity to blue; at others the reverse is true. An obvious corollary of such a shift in relative sensitivity is a reversal of the order of ranking of the color fields as to breadth—a reversal which we have every reason to believe is not pathological. This does not mean, of course, that reversals in the order of ranking as to breadth may not

[1] 'The Absolute Limits of Color Sensitivity and the Effect of Intensity of Light on the Apparent Limits,' PSYCHOL. REV., 1920, 27, 17–19.

be of pathological origin. It does indicate, however, that they may occur in case of the normal eye as the result of no more serious condition than a change in the absolute intensity of the stimulus light, or translated into terms of ordinary office and clinic practice, a change in the intensity of light falling on the perimeter arm at the point of work—a change moreover no greater than may occur in the illumination of a daylighted room from noon to four o'clock on any bright winter day. That is, as the intensity of the stimulus light changes, the limits fall at different distances from the center of the retina. If they happen to fall in a region where the sensitivity is greater to red than to blue, the limits for red will be outside the limits for blue; and conversely if they happen to fall in a region where the sensitivity to blue is greater than to red, the limits for blue will lie outside of the limits for red. There is, moreover, no reason to believe that these irregularities are the same in kind or degree from person to person. Obviously the only safeguard against misinterpretation either in establishing a norm or in making comparisons with an established norm is always to work with the same conditions of control. A difference in result will then mean an actual difference in sensitivity or in the relative distribution of sensitivities, as the case may be. Working under these conditions we have a chance really to find out what is normal with regard to breadth and shape of the fields of sensitivity and their relation for the different colors, and to check up their variations from time to time as result of the advance or recession of a pathological condition.

A further corollary of the change in the ratio of sensitivity to the different colors in passing from the center to the periphery of the retina is the change in hue of red and green. This change is towards yellow when the observation is made with a preëxposure and surrounding field of the brightness of the color and there is a proper control of all other variable factors. If the observation is not made under these conditions the change may be still more complicated. The change towards yellow depends upon two facts: (*a*) The red and green wave-lengths of light have the power to arouse not

only the sensations of red and green but also weakly the sensation of yellow; and (*b*) sensitivity to red and green falls off more rapidly than to yellow in the mid-periphery of the retina. In the center of the retina, which is fully sensitive to red and green, the weakly aroused yellow excitation is below the threshold of sensation, *i.e.*, too weak to be sensed in the presence of the strongly aroused red and green sensations. However, in those parts of the retina in which the loss of sensitivity to red and green is greater than to yellow, the yellow component of the sensation comes above the threshold and the red and green stimuli are sensed as yellowish red and yellowish green. For example, in passing from the center to the periphery of the retina in the nasal meridian, the red stimulus was sensed as red from the center to about 60°, by the observer whose theshold results are given in Charts 1–4; from 60° to about 86° it was sensed as yellowish red or orange; from 86° to the limits of sensitivity it was sensed again as red. Corresponding to this it will be noted (Charts 1–4) that there is in this meridian a fairly close agreement in sensitivity to red and yellow from the center to about 60° (stimulus sensed as red), at which point there is a relatively sharp decrease in sensitivity to red. That is, from 60° to 86° there is much less sensitivity to red than to yellow (stimulus sensed as yellowish red or orange). At about 86° there is a sharp decrease in sensitivity to yellow, and from this point on to the limits, a fairly close agreement again in sensitivity to the two colors (stimulus sensed as red). In the temporal meridian the red stimulus was sensed as red from the center of the retina to about 30°; from there to about 47° as yellowish red or orange; and from 47° to the limits of sensitivity, as red. Similarly in this meridian there is a fairly close agreement in sensitivity to red and yellow from the center to about 30°; from 30° to about 47° there is a considerably greater sensitivity to yellow than to red; and from this point on to the limits greater sensitivity to red than to yellow. In case of the green stimulus in the nasal meridian the greater loss in sensitivity to green as compared with yellow begins about 51°; and in the temporal

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meridian at about 26°. Correspondingly at these points the green stimulus began to be sensed as yellowish green and continued to be sensed in this hue until the limits of sensitivity to green were reached, from which point on for a short distance it was sensed as unsaturated yellow.

These changes of hue are the normal changes for spectrum or pure red and green in passing from the center to the periphery of the retina when, as we have already stated, there is no achromatic effect of preëxposure and surrounding field, that is when the preëxposure and surrounding field are a gray of the brightness of the color and when the general illumination of the room is held constant. In a later paper the changes produced by a preëxposure and surrounding field lighter or darker than the color and by a variable illumination will be given.

The Effect of Variations of the Intensity of the Stimulus on the Size and Shape of the Color Fields and their Order of Ranking as to Breadth.—Results have been obtained for two sets of conditions: high intensities, the fields having been taken with our rotary campimeter and spectrum lights;[1] and medium and low intensities, the fields having been taken with our rotary campimeter and spectrum lights; and medium and low intensities, the fields having been taken with the illuminated perimeter described in a former paper and pigment stimuli.

The results for the first series of determinations have already been given.[2] In the second series of determinations, five intensities of light reflected from pigment stimuli were used: the Hering standard pigment papers under 51, 17, 7, 3 and 0.03 foot-candles of light. In order to show how these illuminations compared with the daylight illumination falling on the perimeter arm in an ordinary room, measurements were made of the daylight at the level of the perimeter arm in a room with southern exposure, six windows the lower sills of which were somewhat higher than the level of the perimeter

[1] 'A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral,' *J. of Exp. Psychol.*, 1916, 1, 247–283; also 'Description of a Rotary Campimeter,' *Amer. J. of Psychol.*, 1912, 23, 449–453.

[2] 'The Absolute Limits of Color Sensitivity and the Effect of Intensity of Light on the Apparent Limits,' PSYCHOL. REV., 1920, 27, 1–24.

arm when placed in the horizontal position. These measurements were made on a bright day, Jan. 14, 1921. It was found that our highest intensity of illumination, 51 foot-candles, was the same as that falling on the perimeter arm at the fixation point when facing one of the windows at 1 P.M.; our second highest illumination, 17 foot-candles, was

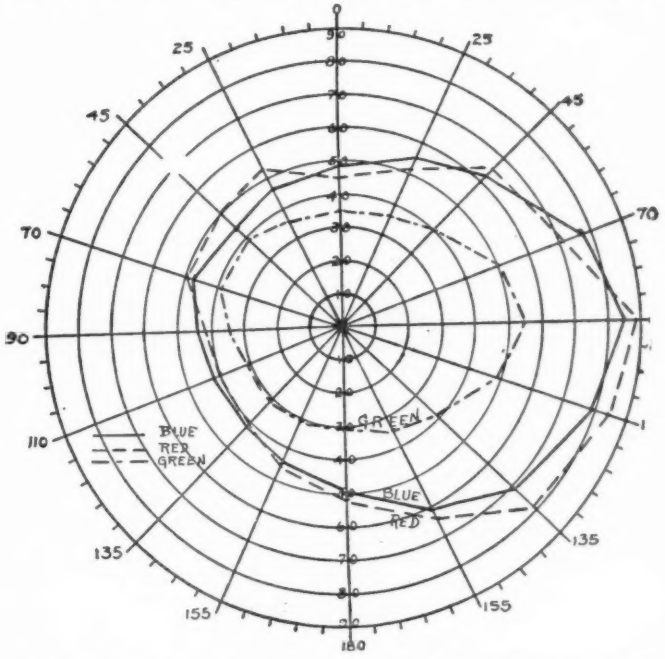

FIG. 1. Fields taken under 51 foot-candles of light—an illumination equal to the daylight falling on the perimeter at the fixation point when facing a window on a bright day, 1 P.M., Jan. 14, 1921.

the same as that falling at the fixation point at 2.30 P.M.; the third highest illumination, 7 foot-candles, the same as that falling on the fixation point at 3.45 P.M.; and the fourth, 3 foot-candles, the same as that falling at the fixation point at 4.15 P.M.

Measurements were also made with the perimeter placed parallel to this window. Our second illumination, 17 foot-candles, was the same as that falling at the fixation point at

11 A.M.; the third, 7 foot-candles, the same as at 1.30 P.M.; and the fourth, 3 foot-candles, the same as at 2.30 P.M. In making the photometric measurements, a test-plate covered with magnesium oxide deposited from the burning metal was set up at the fixation point and its brightness measured

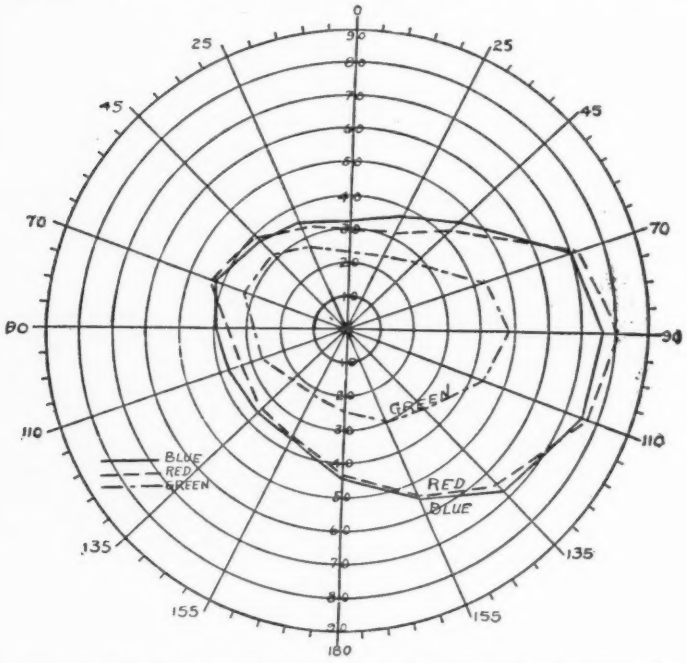

FIG. 2. Fields taken under 17 foot-candles of light—an illumination equal to the daylight at the fixation point, on the same day at 2.30 P.M., perimeter facing window; or at 11 A.M., perimeter parallel to window.

with a Sharp-Millar illuminometer. From these brightness measurements, the value of the incident light in foot-candles was computed. The fields were taken with all of the controls described in our previous paper, 'A New Laboratory and Clinic Perimeter.'[1] They are shown in Figs. 1–6.

The decrease of illumination from 51 to 3 foot-candles narrowed the limits for red by an amount ranging from 11 to 27 degrees; for blue from 13 to 37 degrees; and for green

[1] *J. of Exp. Psychol.*, 1922, 5, 46–67.

from 10 to 19 degrees—a result which it will be remembered was produced by a change of illumination equal to that which occurred from 1 to 4.15 P.M. in a well-lighted room on a bright winter day. The decrease of illumination from 51 to 0.03 foot-candles narrowed the limits for red by an amount

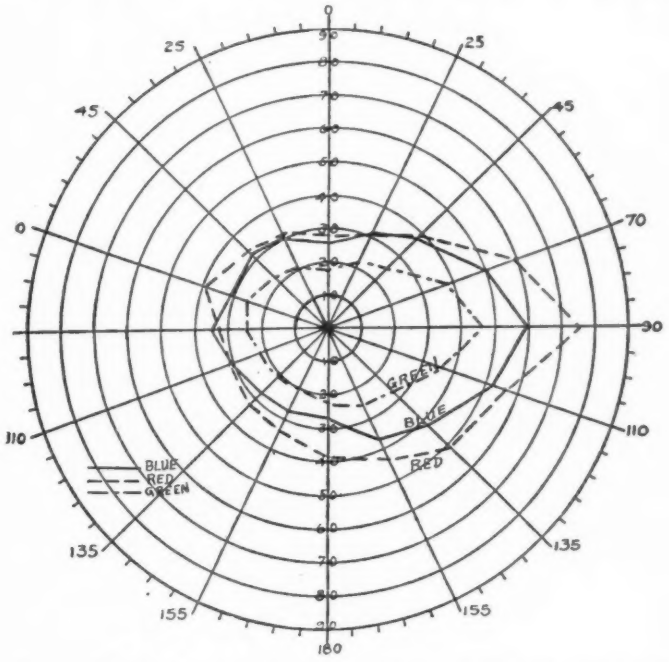

FIG. 3. Fields taken under 7 foot-candles of light—an illumination equal to the daylight at the fixation point, on the same day at 3.45 P.M., perimeter facing window; or at 1.30 P.M., perimeter parallel to window.

varying from 27 to 72 degrees; for blue from 29–61 degrees; and for green from 22 to 45 degrees.

Average Fields for 35 Normal Observers.—Since the perimeter referred to in the preceding section with its pigment stimuli different from those now in use in office and clinic work and its standardization and control of variable factors introduces a new situation in perimetry, it becomes necessary to determine a norm for the color fields under the new con-

ditions to serve as a standard or basis for comparisons. This work is in progress but has not yet been finished. Average fields (Fig. 7) for red, green and blue have been determined for 35 normal observers just as the perimeter stands, *i.e.*, with 17 foot-candles of light on the test-object and with the controls described.

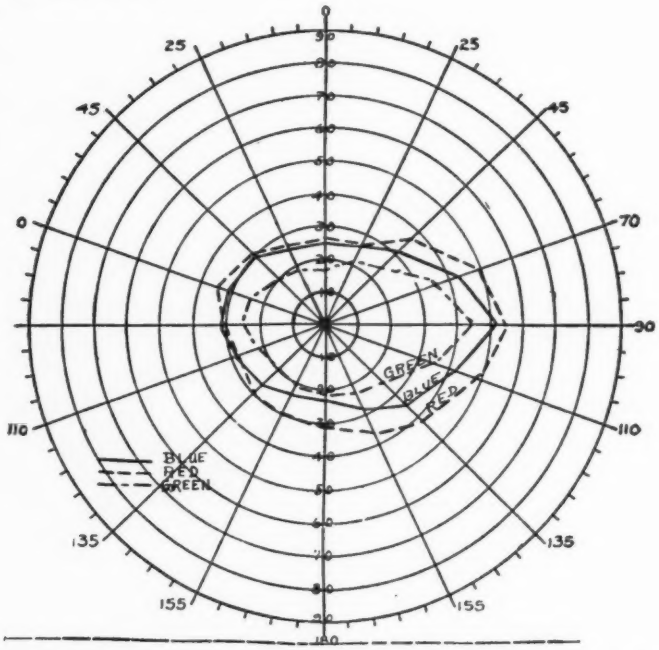

FIG. 4. Fields taken under 3 foot-candles of light—an illumination equal to the daylight at the fixation point, on the same day at 4.15 P.M., perimeter facing window; or at 2.30 P.M., perimeter parallel to window.

The observers used in making the determinations, the average results of which are shown in Fig. 7, were all under 24 years of age. No astigmatisms were present of a value greater than 0.50 diopter; and no myopias of a value greater than 2 diopters. A value of myopia greater than 3.3 diopters would throw the colored stimulus at a distance of 33 cm. (the radius of curvature of the perimeter arm) outside of the

range of accommodation. There were no ophthalmoscopic or other evidences of pathological or abnormal conditions. The intensity of illumination of the stimulus, as already stated, was kept constant at 17 foot-candles. Had the intensity of illumination been different, the fields would of course have

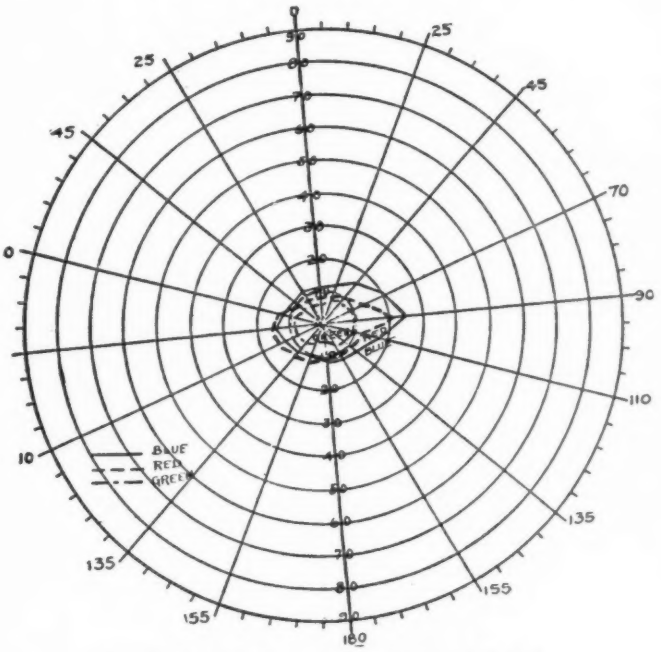

FIG. 5. Fields taken under 0.03 foot-candles of light.

been different. If, as we believe, it will be found advisable to provide for field taking at more than one intensity of illumination, norms will have to be established for each illumination.

The fields given in Fig. 7, it will be noted, are interlacing for red and blue and otherwise differ from those accepted as standard by the medical profession. The interlacing of red and blue is a characteristic of normal limits in the mid-periphery and for periphery of the retina when stimuli of

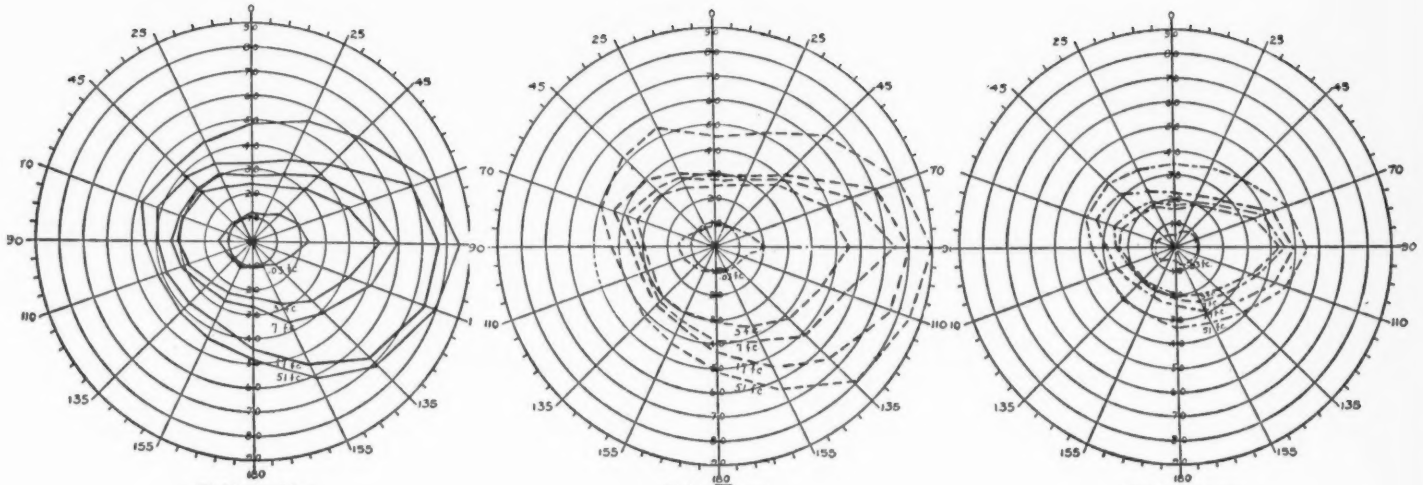

FIG. 6. Showing variations in both the extent and the shape of the color fields when taken under 51, 17, 7, 3 and 0.03 foot-candles of light.

anywhere nearly equal energy are used. The generally accepted clinic rating of the fields as to breadth is in the order from greatest to least of blue, red, green. The interlacing of the red and blue has been claimed by some medical writers to indicate a pathological condition. This may be true for

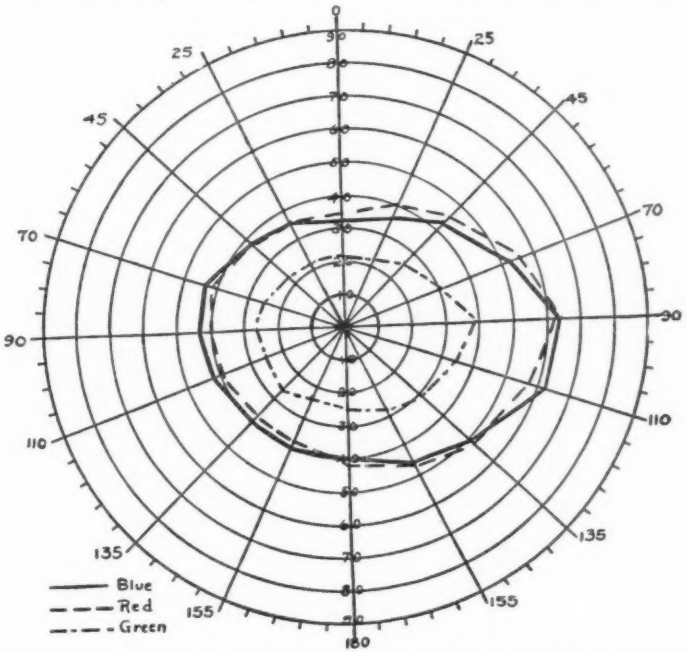

FIG. 7. Average color fields for blue, red and green for 35 normal observers.

the stimuli ordinarily employed by the medical profession, *i.e.*, a dark unsaturated red and a well-saturated blue, and for the conditions under which their work is done. It is not true when stimuli of more nearly equal energy are used and the determinations are made with a proper control of variable factors.

TYPES OF DEXTRALITY

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The subject of dextrality is usually treated as if there were but one type of right-handedness, while in reality there are at least three distinct types of right-handedness with three corresponding types of left-handedness, making in all six forms of dextrality.

The following classification of dextrality according to types is based on the varying manner in which right-handed individuals perform bimanual operations, namely, acts requiring both hands, such as swinging a baseball bat and sweeping with a broom. As an illustration of this variation, we may take two famous baseball players, Tyrus Cobb and Rogers Hornsby. Each of these men is right-handed. Both of them perform such operations as throwing the ball, and other unimanual acts, with their right hands; but in bimanual operations they differ, for Cobb bats left-handed and Hornsby bats right-handed, thus exemplifying two different types of dextrality.

The three types to be discussed will be designated by the symbols *A*, *B*, and *C*. Type *A* comprises those whose bimanual preference is divided between right and left. An individual throwing and batting right-handed, but using a broom or shovel left-handed would exemplify type *A*. Type *B* includes those whose bimanual preference is the opposite of their unimanual preference, and is exemplified by Tyrus Cobb who throws right-handed and bats left-handed. Type *C* embraces the individuals whose unimanual and bimanual preference is the same. The ball player who throws, bats, and also uses a spade right-handed belongs in class *C*. Not knowing how Mr. Hornsby uses such instruments as the shovel and spade, we cannot tell whether he belongs in type *A* or *C*. The same doubt does not exist in Mr. Cobb's case,

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as will be explained later. I have arranged the types in the above order because *A* seems to be the most common and *C* the least common.

Since this classification is based on the bimanual preference, it will be well at this point to discuss bimanual operations in some detail. In designating bimanual dextrality it is generally agreed to call those individuals 'left-handed' who hold the left hand nearer the business end of the instrument. The typical bimanual operations fall into the following regular series, designated by the instrument employed:

ball bat	pitchfork
golf club	shovel
ax	broom
spade	

From extended observation I am sure that the majority of the human race use the broom and shovel left-handed, and the ball bat right-handed; thus coming under type *A*. In the above series the golf club and ax are on the dividing line, and in the order named, for the ax is more likely to follow the broom, and the golf club will follow the bat unless, as often happens when the preference is left-handed, there are no left-handed clubs available. On this account many golf players have had to develop an artificial right-handedness in swinging the clubs. More than any of the other bimanual operations, the use of the ball bat seems to require the same elements of dextrality which enter into unimanual dextrality.

It should also be noted that there is a great variation in the strength of the natural bias. Some individuals are inveterately left-handed, while others are easily converted. Some children have so little preference as to appear practically ambisinistral. The bimanual bias is seemingly independent of the unimanual, and often is much stronger. I myself have very little preference in unimanual operations, but a very decided natural bias in bimanual acts. It makes little difference which hand I use to throw, or hammer, but I must use a spade right-handed. In others it is often the unimanual

bias which is strong and the bimanual which is weak. In the latter case the subsequent development is into type *A*. When both uni- and bimanual biases are naturally strong, and at the same time opposite, the development is into type *B*.

In order to summarize these facts clearly I shall exhibit them in a tabulated form, employing the symbols: *R*, R_1, R_2, *L*, L_1, L_2, and *O*.

R = right-handed, *L* = left-handed, when two symbols are used together, as in the formula *RL*, the first symbol refers to the unimanual preference and the second symbol to the bimanual preference. The dextrality of Tyrus Cobb is then represented by the formula *RL*, because he throws right-handed and bats left-handed, being classified under type *B*. Type *A* is represented by the formula RR_1L_2 because in type *A* the bimanual operations high in the series follow the unimanual preference, while those low in the series are performed on the other side. A man, therefore, who throws right-handed, bats right-handed, and uses a spade left-handed is represented RR_1L_2. *O* represents the weak bias approaching an ambisinistral condition. To illustrate I shall label a few individuals of my acquaintance, arranging them in what seems to be the relative frequency of the occurrence of their respective types.

Name	Probable Natural Bias	Habitual Preference	Resulting Type
John C.	*RO*	RR_1L_2	*A*
John R.	*RL*	*RL*	*B*
Gilbert J.	*RR*	*RR*	*C*
David R.	*OO*	RR_1L_2	*A*
John J.	*OL*	*RL*	*B*
John F.	*LO*	LL_1R_2	*A'*
Andrew P.	*LR*	*LR*	*B'*
Charles H.	*LL*	*LL*	*C'*
Self, natural	*OR*	*RR*	*C*
Self, changed		LL_1R_2	*A'*

John C. throws a ball right-handed, bats right-handed, and pitches hay left-handed; John R. drives a nail right-handed, uses a ball bat left-handed, and shovels left-handed; Gilbert J. did everything right-handed; and so on down the list.

The double representation for myself is necessary on account of the changes I have made as the result of many years' experimenting. At the age of 10 years a broken right arm led me to the discovery that I could eat, or do anything else about as well with my left hand as I had with my right. I immediately became very much interested in the whole subject and tried to see if I could change all my actions, both unimanual and bimanual. There was no difficulty with the unimanual change, except in writing; but the bimanual bias was too deeply seated to make a complete change. It was very little trouble to learn to throw left-handed, but quite difficult to learn the left-handed use of the ball bat and ax, while I have never been able to accustom myself to the left-handed performance of operations low in the bimanual series.

In the relations of uni- and bimanual preference, a curious fact develops which might be anticipated from the results set forth in the above table. There is no such combination as RR_2L_1, or LL_2R_1—stated positively:

(1) XR_1L_2 is always RR_1L_2, type *A*;
(2) RL_1X_2 is always $RL_1L_2 = RL$, " *B*;
(3) RX_1R_2 is always $RR_1R_2 = RR$, " *C*.

As a matter of course, when *R* and *L* are interchanged we get:

$$XL_1R_2 = LL_1R_2 \qquad A'$$
$$LR_1X_2 = LR_1R_2 = LR \qquad B'$$
$$LX_1L_2 = LL_1L_2 = LL \qquad C'.$$

Hence the statement made above, that there is no doubt in the case of Mr. Cobb, for knowing from baseball records (1) that he is RL_1X_2 the *X* is easily determined to be *L* by the above formulæ. When one is seen throwing right-handed and batting left-handed then it is certain that he would pitch hay left-handed.

It will be seen that in type *A* the unimanual preference can be deduced from the observation of one high and one low bimanual operation; in type *B* the low bimanual operation can be deduced from one unimanual and one high bimanual; in type *C* the high bimanual can be deduced from one uni-

manual and one low bimanual. It would be possible by training to develop artificial exceptions to the above equations. This actually happens in the use of scythe and golf club, because left-handed implements are not available to any great extent; but even in such cases it is only for the one operation that the preference is disturbed. The individual who uses a scythe right-handed, contrary to preference, continues as before in all other operations. I have never observed a single exception to the above formulæ, which could not be clearly proved to be artificial.

In this same connection, a hypothesis may be ventured concerning cross-handed batting, being stated tentatively because my observation of this phenomenon has been limited to some ten or twelve individuals. A cross-handed batter is an amateur baseball player who stands on the wrong side of the plate when at bat, if you judge the correct side by which hand is held nearer the outside end of the bat. The player who thus stands on the wrong side of the plate must bat with his wrists crossed, thus shortening the reach of his bat by two or three inches. The one act of cross-handed batting puts an individual in a sub-class of type *B* and indexes completely all his other actions, both unimanual and bimanual. To see a man standing on the north side of the plate and batting cross-handed is to know that in everything else he is strictly *RL*, and that he is so addicted to the left-handed performance of bimanual operations that he will not put his right hand further out than his left even though he prefers to stand on the left side of the plate. Professional players, at least in the major leagues, are not allowed to bat cross-handed and so the natural *RL* cross-hander generally becomes an artificial RR_1L_2, though sometimes he steps to the other side of the plate and remains *RL*, true to type.

The six forms of dextrality existing in practice are then:

1.	RR_1L_2	*A*	
2.	*RL*	*B*	
3.	*RR*	*C*	
4.	LL_1R_2	*A′*	
5.	*LR*	*B′*	
6.	*LL*	*C′*	

and these resolve into the three types:

A.	RR_1L_2	LL_1R_2
B.	*RL*	*LR*
C.	*RR*	*LL.*

B is the ideal type, from the standpoint of symmetrical bodily development.

It should be remarked that not all acts requiring the use of both hands are bimanual in the sense here used. The holding of a rifle requires both hands, and the playing of the violin requires both hands, and normally it is the left hand which is held furthest from the body in each operation; yet in such cases nothing is indicated as to bimanual dextrality, for the act is unimanual as far as dextrality is concerned and the left hand is simply acting under the direction of the right as when it bears the baseball mitt or the warrior's shield. Accordingly the *RR* billiard player will hold his left hand nearer the distant end of the cue, for he is really performing a unimanual operation in which the left hand plays a passive rôle.

The overlooking of the phenomena of bimanual dextrality gives rise to such misstatements as that of Dr. Ernest Jones (2) when he says: "It is exceptional to find a right-handed individual who does not perform some actions better with the left hand." And the failure to observe the types of dextrality may possibly explain some of the failures to obtain definite results in the investigations of the heredity of dextrality. It is undoubtedly hereditary, but that is about all one can safely say. Dr. F. Ramaley (3) concludes that dextrality is probably a Mendelian recessive, but Dr. W. M. Barrows has removed that hypothesis from the field by the discovery of two families in each of which both parents are left-handed, and the children a mixture of right and left. It is possible that a study of pedal dextrality will also be necessary before the heredity question can be cleared up, for the inheritance may go back and forth from hand to foot as in the case of club foot and claw hand.

In the frequent observations made on the 'spade foot' the fact is always overlooked that the foot used in spading

is determined entirely by the bimanual dextrality. Any individual who is XXL_2 will use his left foot on the spade, an occasional cartoonist to the contrary notwithstanding. L. G. Smith (4) says boys always leap on the spade foot. If this be true, then a very important connection has been established between pedal dextrality and bimanual dextrality.

It must be borne in mind, however, that the faculty of springing and kicking are as independent of each other as uni- and bimanual dextrality, and possibly occurring in somewhat the same ratio, that is, the great majority of people kick with the right foot, while the most of them probably spring with the left. My observation of pedal dextrality has been limited, but has always borne out the above statements; so I suggest the probability that the right-handed forms of types *A* and *B* kick with the right foot and spring with the left, while *C* uses the same foot for both actions.

It will be seen from the above remarks that scientific investigations of dextrality should always be accompanied by determination and analysis of the types.

References

1. Star Players' Record. Who's Who in Baseball, 1922, pp. 12, 24.
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THE SIGNIFICANCE OF NEURAL ADJUSTMENT

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Disputes in science spring from many sources. There are disagreements as to proved or probable facts, and differences in interpreting the accepted data. Some controversies hinge on fundamental principles, others are mere questions of definition. Comparing Professor Weiss's conception of the psychologist's problem[1] with that of the present writer,[2] one may well ask how far the clash of opinion is due to each of these causes.

With respect to fundamental scientific attitude the Behavior School and the Eclectic School are in substantial agreement. Both acknowledge the universality of the physicochemical processes, and endeavor to explain the activities of organisms in terms which conform to other natural events. They agree in challenging the uncritical use of such notions as 'vital force' and 'conscious initiative' to explain the mechanism of animal and human response.

My main disagreement with Dr. Weiss appears to rest on our different interpretation of certain facts; he rejects or minimizes the occurrences within the central nervous system as unimportant for psychology, while I regard them as among its most significant data. This difference of appraisal is due in part to the present imperfect knowledge of neural processes, and in part to different conceptions of behavior. Take the latter point first.

What is meant by *behavior?* Behavior is admittedly a certain specific sort of activity of organisms, namely, *response to stimulation.* And it is generally conceded that this specific type of activity does *not* include the creature's growth processes nor any movements or changes within his body which are not mediated by the nervous system.[3]

[1] 'Behavior and the Central Nervous System,' PSYCHOL. REV., 1922, 29, 329–343.

[2] 'Psychology and the Central Nervous System,' PSYCHOL. REV., 1921, 28, 249–269.

[3] Or, in lowest species, by some equivalent intra-organic chain action.

Behavior, as I define it, is the final term of a three-stage process which psychology investigates. The first stage is *stimulation*. The second stage may be called *adjustment;* it is the operation of the adjustor system. The final stage is the *response* proper. Behavior is the creature's total response to any given system of stimuli—that is, to a given *situation*. According to this definition behavior *does not include the mediating neural activity*. 'Adjustment,' the physiological processes that take place in the peripheral nerves and central region, is a distinct sort of activity from 'stimulation' and 'response.' It is the intermediate link in the chain of events.

In other words, psychology is concerned with a certain type of reaction which we may call the 'sensory-cerebro-motor process,' or more concisely the *organic reaction*. This is a three-stage process. Behavior is the final phase of this process. It includes glandular secretion, muscular contraction, and the resulting displacements of various members and parts of the body. In some species behavior includes generation of light or electricity and possibly other physical phenomena. But it does not include the events that occur in the nervous system *before* the contraction or secretion is energized.

It is important at the outset to know whether behaviorists will agree to this definition. For if they broaden the concept of behavior to include neural activity, then part of our dispute proves to be merely a verbal disagreement.

Now for the question of fact. Professor Weiss states that "memory, imagination, thinking, for the behaviorist are parts of *behavior categories* that include an individual-social aspect." [1] But this is just the point at issue. Is thinking essentially an *effector* phenomenon, or is it essentially a *neural* phenomenon? We do not know. The behaviorists believe that thinking is essentially an incipient or implicit operation of the muscles and glands. The introspectionists believe it to be essentially an operation of the central nervous system. In my earlier article it was suggested that we get busy and devise a crucial test to settle the question empirically.[2]

[1] *Op. cit.*, p. 342. Italics are mine.

[2] *Op. cit.*, p. 259.

Since neural conduction occupies an appreciable time (known within limits), it ought to be feasible to determine whether or not a given 'train of thought' includes a greater number of terms than could be produced by successive stimulations. Until this is empirically settled each school is entitled to its own interpretation.

Personally I believe the balance of present evidence supports the introspectionists here. In the first place, the rapid succession of associated thoughts seems incompatible with the view that thought is an affector phenomenon and that each new thought in the series is generated from the preceding by a stimulus-response process. Such a chain of reactions would take too long—it would apparently necessitate a longer delay between successive thoughts than actually occurs. Again, the vividness of dreams seems inconsistent with the torpor of the receptor and effector processes during sleep.[1] In fact the whole array of events to which Weiss alludes ('memory, imagination, thinking') seem easier to understand and account for if we treat them as adjustment phenomena of the observer's central nervous system, than if we treat them as responses—implicit or otherwise.

In any case memory and thought are occurrences of some sort—they are not fictitious. Being real events, they call for scientific examination. And it must not be forgotten that introspection has furnished data in this field which are not attainable by any other known method. The only question is whether in so-called introspection[2] we are observing effector phenomena or behavior phenomena.

Whatever our tentative conclusion, let us beware of supporting it by faulty reasoning. The fact that every observed thought is reported in motor terms is no proof that the thought is itself a behavior (*i.e.*, effector) phenomenon. Undoubtedly when the self-observer reports his experiences he does so in motor terms. But this is true of all scientific

[1] Recently I dreamed of lying in a magnificent bed with a sort of cornice along the sides. On waking I reached out to find which way up the cornice was placed. A real movement occurred. What initiated it?

[2] The term *self-observation* seems more appropriate than *introspection* to describe this research method.

observation. All the data of science are reported in speech or writing. It is a logical error (the 'thermometer fallacy') to identify the phenomenon observed with some secondary indication of the phenomenon. The observer's *report* is merely an indication of his *observation*. To assume that my experience of differentiating two colors is nothing but my verbal or written report of that experience, would establish a dangerous precedent. One might use it to prove that the tropisms of an amœba which the behaviorist observes with a microscope are nothing but the behaviorist's own verbal responses or writing movements when he reports these observations. In suggesting this *reductio* I am challenging the logic, not the facts. For the locus of the observed 'experience' remains to be settled empirically.

Even apart from this undetermined question of fact, it would seem that the behaviorists underestimate the rôle of the central nervous system in determining responsive activity. When I meet a friend I smile, greet him, shake hands. My behavior depends at once on the stimulus (and my receptors), on my effector organs, and on my neural organization. (*a*) I would behave differently in meeting an enemy or if my eyesight were blurred. (*b*) The mode of expression varies if my hands are filled with bundles, or if my vocal organs are impaired. (*c*) But after all is not the chief factor in determining the specific response the *topographical arrangement of my nervous system and its functional development?* Out of a vast number of possible complex responses to the given situation I make a certain one or a certain set. Why? Chiefly (1) on account of a certain arrangement of neurons in my body, and (2) on account of the life-long operation and organization of this system of neurons through repeated stimulation.

1. Certain responses are determined by the original topography of the nerve paths. Suppose a baby bumps his head; he responds by weeping and bawling. The untrained infant does not respond to this sort of stimulus by twitching his toes or scratching his knee. Such facts as these appear to indicate an inherited predisposition toward certain nerve connections and not toward others, despite Mr. Z. Y. Kuo's

argument [1] against the innateness of instincts. Granting the supreme importance of natural selection in building up complex responses, it need not be assumed that the original simple responses are random. Certain nerve paths are quite definitely marked out at birth and before birth. Further, the evidence points to the existence not merely of inherited reflex mechanisms, but of inherited mechanisms for *fairly complex activities*, such as suckling in the human infant, nest-building in birds. These are what we mean by 'instincts.' They are not magical or mysterious powers, as Kuo avers; they are inherited dispositions resulting from the normal line of neural growth in any individual of a given species.[2]

It may be urged nevertheless by the behaviorists that the problem of inherited neural paths can be settled by the behavior method of research alone, without reference to neurology. Very well; but at least should we not call upon neural research to confirm our conclusions? Is it not because the study of nerve topography has discovered the existence of certain tracts and connections that we feel justified in going ahead with our behavior work and drawing these conclusions from its findings?

2. Quite a different problem confronts us when we investigate *acquired* responses. When I meet a friend we do not rub noses, as do certain primitive humans, but we do shake hands. Here the response is acquired, not inherited. It is built up by social practice. The approach of my friend serves not merely as a *stimulus*, but (as Dr. Weiss would say) it serves also as a *signal*, which operates according to the individual-social character of the recipient.[3]

What does this mean? I understand it to mean that with respect to certain frequently recurring stimuli and stimulus-patterns (*i.e.*, situations) a selective process has sifted out the responses, so that in the adult a certain non-inherited response occurs invariably, and in many cases proceeds quite as auto-

[1] Summed up in his recent article: 'How are our Instincts Acquired?' PSYCHOL. REV., 1922, **29**, 344–365.

[2] So far as Mr. Kuo combats the overemphasis of instincts in psychology the writer agrees with him most heartily.

[3] *Op. cit.*, p. 330. I prefer the word *sign* to *signal*.

matically as any inherited reflex or instinct. Accordingly, when a friend says, "Hand me that book, please," I respond with the appropriate movement, whereas the infant would respond to the same stimulus by staring, or blinking, or squalling.

The stimulus serves as a signal or sign when it is conventionally associated with something else. The spoken word 'book' is associated in our community with the object 'book'; the words 'hand me' are associated with a certain form of response; and so on. A stimulus becomes a signal or sign when it carries more than its mere stimulus-value—when it generates something more in the brain centers or in the effectors than it did at the outset.

There are many stimuli besides social situations which serve as signals. The thirsty animal in the desert sees a group of trees and hies him thither. He responds to the tree-stimulus, not as a stimulus in itself, but as the sign of a water hole. The deer responds to a whiff of odor in the wind not as an odor, but as the sign of an enemy. While sailing yesterday I saw a heavy cloud on the horizon and turned my craft homeward to this storm-signal. In short, throughout the animal world we find responses which are appropriate not so much to the stimulus as to something related to the stimulus.

Now in all such cases, if the mode of behavior is really acquired and not inherited, wherein does the learning process consist? I bump my head on a low cellar doorway. Next time I start to go through that doorway, I stoop over and avoid striking the lintel altogether. The rabbit once beset by dogs starts to scurry off next time at the very first dog-odor. This is not trial-and-error learning nor gradual improvement, but an immediate short-cut response. What causes the variation in behavior? How does the selection operate? Does the ultimate explanation lie in changes of central nervous connections, or is there always an intervening incipient reaction which stimulates the new responses of stooping, scurrying off, etc.? Our same old problem, it seems! The question of fact must be solved before we can

gain a clear notion of what the stimulus—nerve-impulse—response process really is.

Meanwhile, it is significant that the simultaneous stimuli due to one and the same object become organized into a group-impression,[1] and that most of our responses are composite and require a well-regulated distribution of the motor-nerve discharge. When I perceive and respond to my friend's presence, it is because a large number of visual stimuli (with perhaps some auditory and other sorts added) affect my receptors simultaneously, so that a complex, integrated neural activity is generated at the centers. When I respond in the customary way to his presence, the response in question is brought about by a complex, coördinated neural activity passing from the centers down certain motor paths. The most characteristic feature of the phenomena which psychology studies is (to my mind) this *molding of the response to the stimulus* by means of the central nervous activity. This neural integration and coördination constitutes the *adjustment* phenomenon. Adjustment includes the entire process of transforming the stimulus energy into the response energy, and the 'pattern' of the complex sensory-nerve impulse into the 'pattern' of the complex motor-nerve impulse. The adjustment process does not always result in an adapted response; the resulting behavior is not necessarily fit or appropriate to the situation. That it tends to become suitable is due to the operation of natural selection.

The behaviorists seem loath to recognize the importance of this neural adjustment process. Possibly they fear that it implies the existence of an 'initiating consciousness' in some disguised form. The present writer is willing to take this risk. He sees no ground for assuming that either 'consciousness' or 'central nervous activity' generate something undetermined by the natural physical antecedents. Granting that behavior is a natural phenomenon which follows the universal 'laws' of all physical activity, nevertheless the adjustment process seems to be the significant factor in

[1] Impression is here used to denote a neural fact, with no necessary implication of consciousnes

determining the specific pattern of behavior in any given instance.

It should not be overlooked that the adjustment process itself depends on stimulation. If a creature forms certain habits, it is because he is confronted by substantially the same situations time after time. His repertory of acquired responses depends upon a fairly constant environment, both physical and social. Weiss is right in insisting on the importance of this factor, though in lower species the social side is apparently far less important than he believes.[1] The investigation of the social and general environmental conditions of organic reaction is surely an important task; but it is not the whole program, nor even the chief problem of psychology.

This brings us back, in conclusion, to our original thesis. We find a certain large group of related phenomena in the life of organisms, namely, the events concerned in the responsive activity of animals to stimuli by means of the nervous system. All these phenomena deserve scientific investigation. They include at once (1) the phenomena of *behavior* or response to stimulation, (2) the *neural processes* which mediate this response, and (3) the phenomena of *conscious life*. These are phases of the same general operation, the 'organic reaction.' Each phase may be investigated by a different method, which utilizes a different set of data. The present writer advocates a psychology which recognizes the validity of at least three methods of research—the behavior method, the neural method, and the method of self-observation. In short, his chief plea is for a broader conception of the province of psychology than either the behaviorists or the introspectionists have hitherto been willing to adopt.

Summary.—This paper is not intended as a reply to Professor Weiss's forceful argument, but to point out certain difficulties in the way of accepting his contentions as representing the whole truth.

(1) We need a clear-cut definition of the term *behavior*.

[1] It must be remembered that the beginnings of behavior appear in non-social creatures.

It is here suggested that we exclude 'neural activity' from its connotation.

(2) An important question of fact remains unsolved: Is imagery—and is thought—primarily a behavior phenomenon, or primarily a neural phenomenon? The final answer awaits empirical evidence. The behaviorist's argument for the first alternative involves a logical fallacy.

(3) The attempt to describe organic reactions solely in terms of the initial and terminal data (stimulus and response) ignores an important link in the chain, namely, neural adjustment. The present writer regards this factor as the key to the entire process.

(4) The individual's experience-life, including his memories and thoughts and especially the phenomena of dreams, constitutes a real class of occurrences and accordingly deserves scientific study. If these events are identified with neural and not with response phenomena, then the behavior method of investigation is incomplete; it should be supplemented by introspection and nerve-study.

No attempt is made in this paper to repeat the arguments for the 'double-aspect' hypothesis. This concept is not significant for those who regard the organic reaction as merely a matter of stimulus and response, and minimize the central adjustment process. Its value to those who accept 'experience' as a phenomenon of the central nervous system need not be insisted upon here.

DISCUSSION

THE AFFILIATIONS OF BEHAVIORISM

I. In the March, 1922, number of the PSYCHOLOGICAL REVIEW a relationship with behaviorism is claimed both for structural and for self-psychology. Self-psychology has already argued that its doctrine is behavioristic both because, like behaviorism, it refuses to limit psychology to the task of analyzing abstract mental processes into structural elements of consciousness, and also because it attributes to the self precisely the characters of the behaviorist's organism (integrated totality, relative stability with growth, and individuality), while it conceives specific experiences, perceiving, feeling, thinking and the like, as the self's responses, or reactions, to the environment both social and physical.[1] This behavioristic aspect of self-psychology is now admitted by Professor James L. Mursell. "The notion of a stimulus-response relation does not necessarily rule out [the] ideal of a self-psychology."[2] Professor Mursell to be sure looks on self-psychology as a mere 'working hypothesis,' quite on a par with behavior psychology. "There appears no reason," he declares, "why all the relevant phenomena cannot be treated either from the standpoint of self-psychology" or "in terms of stimulus-response relation."[3] From this conclusion the self-psychologist of course dissents; and he can point to many 'relevant' psychic phenomena (malice and envy, for example) which are adequately described in terms of personal attitude while indistinguishable in terms of bodily stimulus-response. But more significant than this divergence is Mursell's correct estimate of the essentially behavioristic aspect of self-psychology.

II. In her brilliant President's Address, 'Introspection as an Objective Method,' published in this same number of the REVIEW, Professor Margaret F. Washburn incidentally makes contact with

[1] Cf. Calkins, M. W., 'The Truly Psychological Behaviorism,' PSYCHOL. REV., 1921, 28, 12–18.

[2] Mursell, James L., 'The Stimulus-Response Relation,' PSYCHOL. REV., 1922, 29, 161. For another statement of the relation between self-psychology and behavior cf.—J. S. Moore, "The Foundations of Psychology," p. 85: "Behaviorism and Self-psychology are both developments of Functionalism but in opposite directions."

[3] *Ibid.*, p. 162.

behaviorism from a very different angle. Her paper is mainly occupied with an argument, followed with keen satisfaction by the self-psychologist, leading inexorably to the conclusion that even the behaviorist must extract trustworthy and important results from introspection[1]—notably 'a recognition of the importance of kinæsthetic processes.'[2] But in spite of this disagreement with the behaviorist as regards the method of the science, Miss Washburn finds an important likeness between structural and behavioristic psychology. "Both," she points out, "refuse to allow the interference of non-physical causes in the physical system," and for this reason, she says, "the structural psychologist feels more at home in the behavioristic world of physico-chemically determined behavior than he does in a world where movements are causally determined by . . . active selves."[3]

On this view of the affiliations of these psychological conceptions a self-psychologist may briefly make the following comments. He is inclined to protest in the first place against any approach, whether by animist, by behaviorist, or by structuralist, to the policy of determining one's psychological system by one's philosophical predilections. Hence a preference for exclusively physical causes seems to him a poor reason for denying the experience of self. But in the second place, he objects to the identification of the 'self' of the psychologist with the 'non-physical cause' of the metaphysician. For one may assert, on introspective grounds, the existence of self without believing that the self is directly experienced as cause of anything.

Against a casual criticism, in which once more Miss Washburn allies herself with the behaviorist, the self-psychologist also protests —and with mild amusement. "Both behaviorism and structural psychology," Miss Washburn says, "may well envy functional psychology or self-psychology, their justification in leaving such concepts [as the self-exhibiting instinct, or the inferiority complex] unanalyzed." But this is a caricature of the self-psychologist's position. So far from leaving these conceptions unanalyzed he believes that 'self-exhibition' and 'will to power' (and, for that matter, all experiences) must be 'analyzed' not only after the fashion of the structuralist, but into personal characters or attitudes —activity or passivity, for example, and attention. The self-

[1] PSYCHOL. REV., 1922, **29**, 94.
[2] *Ibid.*, pp. 109 f.
[3] *Ibid.*, p. 90.

psychologist may be right or wrong in this procedure, but he should not be represented as satisfied to stop short of complete analysis. In truth this criticism seems to him to come with peculiarly ill grace from either structuralist or behaviorist. For both are notoriously guilty of an uncritical adoption of the terms of self-psychology without so much as an attempt to analyze the concepts corresponding to these terms into elements of structure or of behavior.

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